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OF THE

American Philosophical Society

HELD AT PHILADELPHIA

FOR

PROMOTING USEFUL KNOWLEDGE

VOLUME LXVIII

1929

THE AMERICAN PHILOSOPHICAL SOCIETY HELD AT PHILADELPHIA
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THE AMERICAN PHILOSOPHICAL SOCIETY
1929

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MINUTES OF THE MEETINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

DURING 1929

Stated Meeting, January 4, 1929

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President, in the Chair

The decease was announced of the following members:

Otto Nordenskjold, Ph.D., at Göteborg, Sweden, June 2, 1928.

Henry B. Fine, A.M., Ph.D., LL.D., at Princeton, N. J., December 21, 1928, æt. 70.

John M. Coulter, A.M., Ph.D., at Yonkers, N. Y., December 24, 1928, æt. 78.

Felix E. Schelling read a paper on "The Elizabethan Theater and Its Personnel in Contemporary Illustration" which was illustrated by lantern slides.

Harold Goodwin presented an informal communication, and presented the Society with an original autographed letter from Samuel Vaughan Merrick describing the "Introduction of Illuminating Gas into the City of Philadelphia."

On motion Mr. Goodwin's gift was accepted with a vote of thanks.

Stated Meeting, February 1, 1929

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President, in the Chair

Dr. Goodspeed announced that on January 11, 1929, the following Committee presented a congratulatory address to President-elect, Herbert C. Hoover:

Francis X. Dercum
Arthur W. Goodspeed
John A. Miller
Albert P. Brubaker
Eli Kirk Price
Charles G. Abbot
Whitman Cross
Arthur L. Day
J. Bertram Lippincott
Leo S. Rowe

President-elect, Mr. Hoover briefly expressed his appreciation.

The decease was announced of the following members:

Paul J. L. von Heyse, Ph.D., at Munich, Bavaria, April 2, 1919.

Joseph Snowden Bell, LL.B., at New York, N. Y., January, 1929.

George O. Trevelyan, at Stratford-on-Avon, England, January, 1929, æt. 90.

Thomas B. Osborne, Ph.D., Sc.D., at New Haven, Conn., January 30, 1929, æt. 70.

James H. McGregor read a paper on "The Present Scientific Status of *Pithecanthropus Erectus*, the Ape-Man of Java" which was illustrated by lantern slides.

Mr. Price presented a report of the Committee on Development.

Pending nominations were read.

Stated Meeting, March 1, 1929

Francis X. Dercum, M.D., Ph.D., Sc.D., President, in the Chair

A. S. W. Rosenbach, recently elected member, subscribed the Laws and was admitted into the Society.

The decease was announced of the following members:

W. Boyd Dawkins, Kt., M.A., Sc.D., at Manchester, England, January 15, 1929, æt. 91.

Charles Custis Harrison, A.M., LL.D., at Philadelphia, Pa., February 13, 1929, æt. 85.

Herbert Fox read a paper on "Some Observations on the Comparative Constitution or Biological Peculiarities in Man and the Lower Mammalia" which was illustrated by lantern slides and discussed by William B. Scott and Witmer Stone.

The Committee on Nominations of Officers made its report.

General Stated Meeting, April 18, 19, 20, 1929
Thursday Afternoon, April 18th
Opening Session, 2 o'clock

Francis X. Dercum, M.D., Ph.D., Sc.D., President, in the Chair

Eldridge R. Johnson, recently elected member, subscribed the Laws and was admitted into the Society.

The decease was announced of the following members:

Samuel Castner, Jr., at Philadelphia, Pa., March 3, 1929, æt. 85.

Samuel Rea, Sc.D., LL.D., at Philadelphia, Pa., March 25, 1929, æt. 74.

The following papers were read:

"The Origin of the Diamond in Nature," by Monroe B. Snyder, Director Emeritus of the Philadelphia Observatory.

"The Tomb of the Moghul Emperor Babur in Afghanistan," A. V. Williams Jackson, Professor of Indo-Iranian Languages, Columbia University.

"The Concept of Nature in Philosophy and Literature; a Consideration of Recent Discussions," by Albert Schinz, Professor of French, University of Pennsylvania. Introduced by Dr. Frank. Discussed by Harrison Morris.

"Railroad Consolidation," by Emory R. Johnson, Professor of Transportation and Commerce, University of Pennsylvania.

"Old World Prehistory in Retrospect and Prospect," by George Grant MacCurdy, Research Associate in Prehistoric Archæology and Curator of the Anthropological Collections, Yale University. Discussed by William B. Scott.

"The Special Contribution of Developmental Mechanics to the Thought and Purpose of the Man of Tomor-

row," by Oscar Riddle, Carnegie Institution, Cold Spring Harbor, New York. Discussed by Edwin G. Conklin.

Friday, April 19th

Executive Session, 9:30 o'clock

Francis X. Dercum, M.D., Ph.D., Sc.D., President, in the Chair

The President delivered his annual address and appointed the Committees on Nominations and General Meeting.

Charles H. Warren, recently elected member, subscribed the Laws and was admitted into the Society.

The Library Committee submitted the following resolutions which were approved:

Resolved that it is the sense of the Library Committee that our invaluable Manuscripts should not be taken from the Society's Hall for photostatic or other purposes, but that the Manuscripts may be photostated provided the apparatus can be brought to the Society's Building.

That the Library Committee wishes to call the attention of the Society as a whole to the inadequate protection from fire or theft of its priceless collection of Manuscripts and Books and to urge that the Society take action at as early a date as possible, to provide more adequate protection for these treasures, the loss of which would be irreparable.

Morning Session, 10 o'clock

Francis X. Dercum, M.D., Ph.D., Sc.D., President, in the Chair

The following papers were read:

"Angular Scattering of Electrons by Gas Molecules," by Gaylord P. Harnwell, National Research Fellow, Princeton University. Introduced by Dr. K. T. Compton. Discussed by Monroe B. Snyder and Arthur E. Kennelly.

"Gudermannians and Lambertians with their Respective Addition Theorems," by Arthur E. Kennelly, Professor of Electrical Engineering, Harvard University.

"Gravitation," by Charles F. Brush, Electrical Engineer.

"The Composition of the Sun's Atmosphere," by Henry Norris Russell, Professor of Astronomy, Princeton University. Discussed by Charles G. Abbot.

"The Coma-Virgo Galaxies," by Harlow Shapley, Director of the Harvard Observatory, and Adelaide Ames. Discussed by Henry N. Russell.

"Navigational Methods and Aerial Photography in Polar Exploration," by O. M. Miller, Instructor in the School of Surveying, American Geographical Society. Introduced by Dr. Bowman. Discussed by Henry G. Bryant.

Afternoon Session, 2 o'clock

HENRY H. DONALDSON, Ph.D., Sc.D., in the Chair

The following papers were read:

"Similar Sugars," by Claude S. Hudson, Professor of Chemistry, Hygienic Laboratory, U. S. Public Health Service. Introduced by Dr. Stieglitz.

"An Experimental Investigation of the Physical Nature of Death," by George W. Crile, Director of the Cleveland Clinic and of the Cleveland Clinic Hospital Discussed by Charles G. Abbot and John W. Harshberger.

"The Nutritional Properties of Malignant Cells," by Alexis Carrel, Member of the Rockfeller Institute for Medical Research.

"The Vegetation of Campos do Jordão, Brazil," by John W. Harshberger, Professor of Botany, University of Pennsylvania.

"Lindeniopiper, A Generic Segregate from Piper," by William Trelease, Professor of Botany, University of Illinois. (Read by title.)

"John Bradbury, the Botanist" by Rodney H. True, Professor of Botany, University of Pennsylvania.

"Segmental Interchange, a Possible Basis of Chromosomal Attachments in *Oenothera*," by Albert F. Blakeslee, Assistant Director in Plant Genetics, Carnegie Station for Experimental Evolution, Cold Spring Harbor, and R. E. Cleland. Discussed by George H. Shull, Bradley M. Davis and R. E. Cleland.

Friday Evening Lecture

Aleš Hrdlička, Curator, Division of Physical Anthropology, U. S. National Museum, spoke on "The Future of Man in the Light of His Past and Present." (Illustrated.)

Saturday, April 20th

Executive Session, 9:30 o'clock

Stated Business, Election of Officers and Members

Francis X. Dercum, M.D., Ph. D., Sc. D., President, in the Chair

The Society proceeded to an election of Officers and Members.

The Tellers subsequently reported that the following officers and members had been duly elected:

President

Francis X. Dercum

Vice-Presidents

William W. Campbell
James H. Breasted
Elihu Thomson

Secretaries

Arthur W. Goodspeed
John A. Miller

Curator

Albert P. Brubaker

Treasurer

Eli Kirk Price

Councillors

(To serve for three years)

Charles B. Davenport
William H. Hobbs
Emory R. Johnson
Harlow Shapley

Members

William F. Albright
Harley H. Bartlett
George Henry Chase
James Pyle Wickersham Crawford
William Darrach
Clinton J. Davisson
Charles Hall Grandgent
John Louis Haney
E. Newton Harvey
Edwin P. Hubble
William Jackson Humphreys
Solomon Lefschetz
James Howard McGregor
Michael I. Rostovtzeff
Frank W. Taussig
Owen D. Young

Morning Session, 10 o'clock

Albert P. Brubaker, A.M., M.D., LL.D., in the Chair

The following papers were read:

"Some Fundamental Biological Problems in the Development of an Organism," by J. H. Bodine, Assistant Professor of Zoology, University of Pennsylvania. (Introduced by Dr. McClung.) Discussed by Clarence E. McClung.

"A Chemical Basis of Growth by Cell Division," by F. S. Hammett, Professor of Research, The Research

Institute of the Lankenau Hospital. Introduced by Dr. Dercum. Discussed by Archibald B. Macallum.

"On the Growth of Mammalian Molar Teeth after Eruption," by Henry H. Donaldson, Professor of Neurology, Wistar Institute of Anatomy.

"Mean Sealevel Studies in New York Waters," by Douglas Johnson, Professor of Physiography, Columbia University. (Read by title.)

"A Mounted Skeleton of Palaeonictis," by William J. Sinclair, Associate Professor and Curator, Princeton University, and G. L. Jepsen. Discussed by William B. Scott.

"The Carcasses of the Mammoth and of the Rhinoceros found in the frozen ground of Siberia," by I. P. Tolmachoff, Curator, Carnegie Museum of Pittsburgh. Introduced by Dr. Osborn. Discussed by William B. Scott.

"Some Educational Values of the American Revolution," by Evarts B. Greene, Professor of American History, Columbia University. Introduced by Professor Lingelbach.

"The Development of Brain Correlates in the Evolution of Animal Behavior," by Frederick Tilney, Professor of Neurology, Columbia University. Introduced by Dr. Osborn. (Read by title.)

"The Metabolic Gradient in Animals," by George H. Parker, Professor of Zoology and Diresor of the Zoological Laboratory, Harvard University. (Read by title.)

Afternoon Session, 2 o'clock

Francis X. Dercum, M.D., Ph.D., Sc.D., President, in the Chair

The following Symposium on "Which is the Earlier Civilization?" was presented:

"The Antiquity and Character of the Egyptian Civilization," by James H. Breasted, Director of the Oriental Institute, University of Chicago.

"The Antiquity and Character of the Babylonian Civilization," by Leon Legrain, Curator, University Museum, University of Pennsyvania. Introduced by Professor Barton.

"A Comparison of Egyptian and Babylonian Civilization and their Influence on Palestine," by Alan Rowe, Excavator of Beisan. Introduced by Professor Barton.

"The Origins of Civilization in Africa and Mesopotamia, their Relative Antiquity and Interplay," by George A. Barton, Professor of Semitic Languages, University of Pennsylvania.

The Symposium was discussed by Aleš Hrdlička and George G. MacCurdy.

Saturday Evening

The Annual Dinner was held at the Bellevue Stratford Hotel.

The Toasts responded to were as follows:

"Learned Societies and Museums of Research," James H. Breasted

"Our Universities," Josiah H. Penniman

"Franklin," St. George L. Sioussat

"The Opportunity of the American Philosophical Society," Harlow Shapley

Stated Meeting, November 1, 1929

Francis X. Dercum, M.D., Ph.D., Sc.D., President, in the Chair

John Louis Haney, William Jackson Humphreys and Paul Philippe Cret, recently elected members, subscribed the Laws and were admitted into the Society.

The decease was announced of the following members:

Stewart Culin, at Brooklyn, N. Y., April 8, 1929, æt. 71.

Charles E. de M. Sajous, M.D., LL.D., Sc.D., at Philadelphia, Pa., April 27, 1929, æt. 77.

John W. Harshberger, A.B., B.S., Ph.D., at Philadelphia, Pa., April 27, 1929, æt. 60.

Rodolfo Lanciana, Ph.D., LL.D., D.C.L., at Rome, Italy, May 22, 1929, æt. 82.

Charles F. Brush, M.E., Hon. M.S., Sc.D., Ph.D., LL.D., D.Eng., at Cleveland, O., June 15, 1929, æt. 80.

Hampton L. Carson, M.A., LL.D., at Philadelphia, Pa., July 18, 1929, æt. 77.

Frank Austin Gooch, A.M., Ph.D., at New Haven, Conn., August 12, 1929, æt. 78.

Edwin Ray Lankester, K.C.B., M.A., Sc.D., LL.D., at London, England, August 15, 1929, æt. 82.

George P. Merrill, Ph.D., Sc.D., at Washington, D. C., August 16, 1929, æt. 75.

Thomas Lynch Montgomery, A.B., Litt.D., at Philadelphia, Pa., October 1, 1929, æt. 67.

John A. Miller read a paper on "The Solar Eclipse of 1929" which was illustrated by lantern slides and discussed by Monroe B. Snyder, W. F. G. Swann and William J. Humphreys.

A paper on "The Nature of Thought and Its Limitation" by Francis X. Dercum was read by title.

The President announced that owing to the deaths of Councillors Charles F. Brush and Hampton L. Carson, there were two vacancies on the Council.

The Secretary therefore nominated William B. Scott and John F. Lewis as Councillors for 1929–30.

The President announced that owing to the death of Thomas Lynch Montgomery, there was a vacancy on the Committee on Library.

The following resolution was recommended by Council at its special meeting held on October 24, 1929:

"That the Council recommend to the Society that the Curator and the Chairmen of the Committees on Library and Hall constitute a Committee with power to add to their number and that this Committee be authorized to place the most valuable documents and works of Art of the Society in appropriate places of safety."

The above resolution was approved by the Society.

Stated Meeting, December 6, 1929

FRANCIS X. DERCUM, M.D., Ph.D., Sc.D., President, in the Chair

James Pyle Wickersham Crawford, recently elected member, subscribed the Laws and was admitted into the Society.

The decease was announced of the following member:

Victor Clarence Vaughan, M.D., Sc.D., LL.D., at Ann Arbor, Mich., November 21, 1929, æt. 78.

Elihu Thomson read a paper on "Why the 200-Inch Telescope?" Harlow Shapley read a paper on the "Ten Unsolved Mysteries of the Stellar Universe." These papers were discussed by John A. Miller, Monroe B. Snyder and John F. Lewis.

Through the generous cooperation of the National Broadcasting Company the above addresses were broadcast over an extensive net work of stations with very satisfactory results.

William B. Scott and John F. Lewis were unanimously elected Councillors to fill the vacancies on the Council.

Walton Brooks McDaniel was appointed a member of the Committee on Library to fill the vacancy on that Committee.

The minutes of the Stated Meeting of the Council held on November 15th were submitted.

The Annual Report of the Girard Trust Company was presented and referred to the Committee on Audit.

PROCEEDINGS

OF THE

AMERICAN PHILOSOPHICAL SOCIETY

HELD AT PHILADELPHIA

FOR PROMOTING USEFUL KNOWLEDGE

Vol. 68 1929 No. 1

MAN'S FUTURE IN THE LIGHT OF HIS PAST AND PRESENT

By ALEŠ HRDLIČKA

(Read April 19, 1929)

No subject has occupied human attention more than the future of the individual after death; but the amount of thought given to the terrestrial future of man has been astonishingly small. Even now, thoroughly conscious as scientific workers are of human evolution in the past, they seldom attempt to picture what may happen with man during the endless stretches of time ahead of him or to give more than a sketchy reference to this future.

The reason is that sound speculation in this direction would not only have to be based on a most thorough possible knowledge of early man morphologically as well as otherwise, which but a few can possess; but that so far as the more distant future of man is concerned the visualization of the same would require a genius of invention. For the distinctive features of these more distant aspects of humanity there is no parallel or example in the past. There will be new conditions which can not be foretold except perhaps in part through some genial inspiration.

Considering the subject from their special points of view, some scientists have in recent years expressed their opinions as to man's nearer future, and they reached varying attitudes

(23)

towards the problem. One of the most esteemed of our living biologists, Edwin Grant Conklin, in his "Direction of Human Evolution" (1921, 56, 245), gives voice to the idea that the limits of physical evolution may have been reached, at least for the present, but that there are before us still vast possibilities of social evolution, with such mental advance as will bring mankind as a whole "somewhat nearer to the level of the best individuals of the past and present." Henry Fairfield Osborn, the foremost living American paleontologist and for many years now also a student of man, has not given us as yet his ripe conclusions as to man's future, but his attitude is known to be hopeful.

More commonly, especially among the philosophers and the nonscientific or scientists in other lines than that of man's natural history, we meet with thoughts of apprehension. The eugenists see the danger in the unrestrained propagation of the lower classes. To the vital statistician the danger lies in a general overpopulation of the world. Some of the literary men apprehend an eventual failure of the white and domination of the colored races. While some humanitarians and others fear a degeneracy, or a coming senility, of the race.

All these are essentially advancing thoughts, ideas, theories. They are comparable with the philosophies of the past. They doubtless embody more or less of realities, but they are not rigid simple scientific inductions. Such inductions, as far as they may be possible at the present time, must be arrived at purely by the study of facts. And these facts are the evidence of man's physical and intellectual progress in the past, with those that may be discernible at the present.

Had we a perfect knowledge of the human past, our whole mental attitude towards the human problem would be altered. We should much more fully understand ourselves, could much better appreciate and weigh the changes in man now going on, and could to a considerable extent deduce validly at least the nearer future of the human species.

Regrettably the past of man is still far from a plain or complete reading. A large majority of important details are

still missing or obscure. But through all this haze there are already discernible the outlines of the essentials, and these teach important truths.

They teach that man in something like his present form is relatively but a recent comer in this world. They show that he approached the status of present man within less than thirty thousand years, which is less than one tenth, possibly less than one twentieth of his existence. And they demonstrate that before this both physically and mentally he was, as we progress backward, less and less like his present types, until towards the beginnings of the Ice Age it is impossible to say whether he was already a Man, or still but a primate Precursor.

The general knowledge of man's past is no more a lot of ingenious assumptions or inductions, but a great body of well founded facts, documented on the physical side by scores of early human skeletons or their parts, on the mental or "cultural" side by millions of stone and other artifacts, and chronologically by vast numbers of bones of extinct animals. The evidence is easily accessible to all in the Old World, the prime home of early man, particularly in western and central Europe, North Africa and Asia Minor, in institutions, in the once inhabited caves, rock-shelters and sites, in the glacial river terraces and deposits.

The essentials of the knowledge of man's past are as follows:

Man, in origin, is not apart of but belongs to the rest of the living world.

Man's coming into existence and his further differentiation resulted not from any of his volition or intentive activities, but from conditions inherent or evolving in nature and acting through the forms from which he developed. He is truly a product of Nature.

Man has developed, in all probability gradually, from the nearest subhuman forms, and under the *exciting* influence of environmental conditions. He then progressed gradually, though doubtless not regularly or at the same rate, towards

his present status. During this progress he differentiated into numerous types and races, the less successful of which have become extinct. He is still substantially attuned to Nature, though the relation is becoming weakened through his artificialities.

Man began physically and mentally much inferior to what he is today even in his most primitive present states. The reason of why he began is involved with the general great question as to the why of the beginning of every other important biological form and of life itself; it can not yet be answered. Demands of and responses to complex environment were doubtless among the strongest of the immediate agencies. The advancing glaciation may have been one of the main determining factors. But the ultimate, fundamental causes of man's origin are not as yet graspable.

Human infancy was very long. If man exists for 350,000 years, which according to present knowledge would be a moderate rather than exaggerated estimate, then his infancy and childhood may be said to have taken over three hundred thousand years. The progress was slow, very slow, yet in general and for the main stream, it was always a progress, never a regression, except perhaps in small individual contingents. The great wonder is how man sustained himself during this long, dangerous, almost helpless period of his dawning abilities and consciousness; and how he rose in potentiality until, during the latter part of his Neanderthal phase, he overcame the vicissitudes of the last severe ice invasion, together with the incubus of his old inferiorities, and rose from it to his post-glacial form, that of *Homo sapiens*.

Up to the end of the last main glaciation, man progressed evidently but very slowly in numbers. In general he did seemingly but slightly better than to sustain himself. His spread was slow and sparse. But after the main part of the last glacial invasion he begins to multiply much more effectively and as his numbers increase there follows gradually a spread all over the habitable earth, with an accentuated differentiation of types and races. The latter proceeded all,

according to the best evidence, from but one human species, and those now living date all from the later, post-glacial, parts of human prehistory; earlier strains, such as doubtless there were, can no more be traced among living men.

The main phenomena of human differentiation or "evolution" throughout the past, are on one hand a progressive mentality, on the other hand a progressive physical adaptation and eventually refinement. It is a wonderful and, in general, sustained progress from a more-or-less ape-like precursor to the highest type of man and woman of today. In addition to this is noticed a gradually growing conquest of the environment. Cold, heat, storm, famine, the marsh, forest, mountain, the sea, the wild animals, all the friendly as well as the adverse agencies, even disease, are more and more effectively understood and mastered, or guarded against.

Such, in rough lines, is the past. It is a long period of slow, doubtless uneven, yet ever sustained progress, from an estate less than human to that of the already largely liberated modern man.

When we impersonally observe the present, it is seen, notwithstanding its great complexities, to be merely a developing continuation of the past. Man is still, it appears, as plastic in body and mind as he ever was, probably even more so; he is still struggling with environment, though controlling it more and more every day; and he still changes.

He lives longer and better. He suffers less physically. Elimination of the less fit has largely changed to elimination of the unfit only, the others being given time and chance for restoration through the ever active *vis mediatrix naturae*.

Less mother's and child's hard labor, more and better food, with exercise, sport and personal hygiene, are bringing about an increase in stature of man in the civilized countries, America in the first place; while less use of the jaws and muscles of mastication, due to better prepared foods, is reducing the teeth, the jaws, the breadth, protrusion and massiveness of the face. The head in general among the cultured is becoming slightly broader and larger, the skull

and facial bones thinner, the physiognomy more lively and expressive.

The features, the hands, the feet, are becoming more refined, and general beauty is on the increase, in both men and women. The sensory organs and centers, particularly those of sight, hearing and taste, are evidently growing more effective as well as more resistant. And there is unquestionable advance in civilized man of mental effectiveness and mental endowments. Records in endurance and in accomplishments are ever being surpassed, and modern commerce, industry, finance, science, applied arts, are bringing to light a mental giant after giant.

The historic human genii alone, these great intellectual sky-rockets, are not yet being exceeded, they in their specialties rose too high to be overtaken in such a relatively short period.

Those and other progressive changes in the cultured man of the present are resulting, it is true, in various weakenings and consequent disorders. The hair, especially in the men, is being lost prematurely; the teeth are weakened in resistance, there are troubles of eruption, and some of the dental units tend to disappear. The facial changes, while favoring a greater variety and a higher range of the voice, lead often to disturbing irregularities of the nasal structures and palate. The weakenings through less use of the feet and other organs (appendix, muscles, etc.) result in difficulties, even dangers. Great mental application favors digestive and other disorders; etc. But all these disadvantages are being checked by new adaptations and have but a moderate effect of retardation on the general evolutionary progress of civilized man.

The uncivilized human groups are being awakened whereever possible and urged on to such progress as may be possible to them; but many advance only tediously, and some stay more or less stagnant.

On the whole the lesson of the present is that environmental influences on man have been largely neutralized, and replaced by powerful social and other factors of man's own

direct or indirect make; and that further differentiation of man progresses in accordance with these new influences and demands, as far as they reach geographically and individually. But the road is not smooth and easy. And large contingents of humankind stay more or less behind, which accentuates, rather than levels, group differences.

Such, in high lights, was man's past and such is his present, in evolution. The important problem before us is what on the basis of what preceded and what is now observable, may with approximate safety be expected for the future. For this matters greatly to human hope, attitude, duty, behaviour. The best and safest means towards this end will be strict scientific logic.

The first major appreciation in our quest is that of a limitation. There is, as has already been said, an utter impossibility, and that even if we should draw most freely on imagination, of confidently judging of the distant human future. The inductions now possible can not apply to more, at most, than a few thousands of years. And even for this period much must remain uncertain; nevertheless there may be made a fair beginning.

In general man's past and present permit the statement that he is not yet perceptibly near the end of his evolution, and the prediction that, according to all indications, he will for long yet keep on progressing in adaptation, refinement and differentiation.

But this applies only to the main stream of humanity, the civilized man. The rest will be more or less brought along, or left behind. So far as can be discerned there is no promise of eventual equality of races, and the gulf between the front and the back ranks will probably increase rather than decrease. There will be always masters and servants, the pioneers of progress and the drags.

The progress of the advancing parts of the race may be foreseen to be essentially towards ever greater mental efficiency and potentiality. It will be, in other words mainly a further differentiation and refinement of the brain and of the

sensory nervous system. For it is the factors that call for such further developments that predominate ever more in the modern world. The further mental developments may be expected to be attended by an additional increase in brain size; but this gross increase will, as witnessed with superior brains today, be of but moderate proportions. The main changes will be in the internal organization of the brains, in a greater blood-supply, a greater general effectiveness.

The skull will in all probability be still thinner than it is today. This partly because the enlarging brain, but mainly because due to still further expectable dimunition of the stresses of the muscles of mastication a reason for thickness of the vault shall have diminished. And the skull may on the whole be expected to grow fuller laterally and also antero-posteriorly, due to developments in the directions of least resistance. The hair of the head, the indications are, will probably be further weakened.

The stature promises generally to be even somewhat higher than today among the best nourished and least repressed groups. But there is no indication as yet that it may reach what today would be termed giantism.

The face will, it may be expected, proceed slowly in refinement and handsomeness and character. This partly through intensifying intelligent sexual selection, partly through further reduction of the bony parts consequent upon diminished mastication, and partly through the further development of the frontal portion of the skull. The eyes will, it is plain, be rather deeper set, the nose prominent and rather narrow, the mouth still smaller, the chin more prominent, the jaws even more moderate and less regular, the teeth tending to smaller, diminished mostly in number, even less regular than now in eruption and position, and even less resistant. The future of the beard is uncertain, but no such weakening as with the hair of the head is as yet observable.

The body will tend to slenderness in youth, the breasts towards small, the pelvic parts but little affected, the lower limbs towards long, the upper rather towards short, the hands

and feet towards narrower, the fingers and toes towards more slender, with the fifth toe probably further diminishing.

As to the internal organs, the only more plainly foreshadowed probabilities are a further weakening and diminution of the appendix, and a shortening, with diminution in capacity, of the intestines. As food may safely be expected to continually be more refined and made more digestible, the necessity of a spacious large intestine will diminish in proportion.

Physiologically, the tendencies indicate a rather more rapid than slower pulse and respiration with rather slightly increased than decreased temperature—in other words a livelier, rather than more sluggish, metabolism. But substantial changes in these as well as in other organic functions are not to be anticipated for many millenniums; these functions are too firmly established.

So much for normal conditions. There is, regrettably, also the debit side to be considered. Man has ever paid for his advance, is paying now, and will pay in the future. Functional disorders, digestive, secretive, eliminative, disorders of sleep and sexual, can not but multiply with the increasing stresses, exertions and absorptions. Mental derangements will probably be more frequent. Destructive diseases such as diabetes, and various skin troubles, will probably increase until thoroughly understood and hindered. The teeth, the mouth, the nose, the eyes and ears, will ever call for an increased attention. The feet will trouble.

Childbirth will not be easier nor less painful; though assistance will equally rise in effectiveness.

Due to prolonged life, heart troubles, apoplexies, cancer, and senile weaknesses of all sorts, will tend to be more common, until mastered by medicine.

All this, with many abnormal social factors, will retard but not stop man's progress, for the indications are that he will rise equal to all his growing needs as they develop and begin to hurt.

There is no life-danger to humankind to be apprehended on these scores.

If there is a danger to human future, it lies in the birth rate of the torch-bearers. Already now the birth rate in the families of the most intellectual is unsatisfactory, and this is not due solely, or perhaps even largely, to intentional prevention. Large brains and large families do not go well together. The causes of it have not yet been sufficiently studied and thus are not understood. Some adaptation or compromise may eventually become manifest. If not, it will mean that the talents and genii will in the main have to keep on rising from the lower strata, as they have done hitherto. Could the best human products effectually propagate among themselves, the way towards the differentiation of a separate higher stratum of man would be open, with all its bright as well as dire consequences. This might, however, be too fast a progress for nature as well as man himself.

A much slower but safer and probably sounder progress in further human evolution will be aided materially by the ever advancing true enlightenment of the masses. Enlightenment particularly in all that is favorable and all that is unfavorable towards evolutionary progress of man. In furthering this the science of man will join hands with the empirical practical eugenics of religion, and with scientific eugenics based on researches in heredity and genetics and on biological knowledge in general.

So much, in the main, for the more proximate future of man.

To summarize: Safe deductions from the knowledge already in our possession are that man has developed gradually from forms closely related, but lower than human; that the most important part of the process was his mental evolution; that he is still evolving where conditions are favorable, physically and especially again mentally; and that there is a practical certainty of his further evolution, once more mainly in the intellectual direction, in the perceivable future.

To try to fathom what will happen with man in the distant future would be to lose ourselves in unwarranted speculation. Only a few facts seem certain. As man will advance in

knowledge, so he will advance in the understanding of what is truly advantageous and what truly disadvantageous to him, which will make it easier for him to follow the right road. He will advance in the control of nature, which will aid him greatly in shaping his own destinies. And he will ever more understand disease with its antidotes and immunization. With these gains in perception and power, notwithstanding the many difficulties in the way and the many less fit that must be left behind, a long human progress appears assured.

THE VEGETATION OF THE SCREES, OR TALUS SLOPES OF WESTERN NORTH AMERICA

By JOHN W. HARSHBERGER

(*Read by title April 18, 1929*)

Through the Danish-Swedish *skred* the word scree goes back to the Icelandic *skritha*, which implies a landslip on a hillside. It occurs in Icelandic local names, one of which *strithu-fall* means an avalanche, for some kinds of screes are formed by avalanches. The idea conveyed by the word *skritha* is to creep, crawl, glide, move and is related to the Anglo-Saxon *scrithan*, go. The English word *scrithe* means to stride, to move forward. A *scree* is a pile of debris at the base of a cliff. It is talus, or a slope of rocky fragments, which lies below a precipice from which pieces are broken off, and fall down to the base of the cliff. *Talus* is rubble, or rock rubbish and trash representing rough stones of irregular shapes and sizes broken from larger masses by the natural weathering of the rocky strata above. In New England, as on the slopes of Mount Katahdin, *a slide* is a landslip of loose rock fragments. The word *shingle* is applied usually to the coarse gravel, or loose angular stones of a sea beach, where the idea of a crunching sound when walking over it is implied. It is applied occasionally to talus, as in the combination shingle-slopes. In Devonshire, England, great masses of broken rocks sprawling down the mountain slopes are called "*clitters*" or "*clatters*" (Welsh *clechr*). Lava flows which in Mexico form the so-called *pedregal* are recalled, but they are very different from talus, although volcanic scoria at times may have a somewhat similar character to talus. The German word *schutt*, for talus, means literally refuse, rubbish, rubble and appears in various combinations as *schutt-damm*—earth-bank, embankment. *Geröllfluren* and *schuttfluren* may be

translated rubble grounds, while *Schuttflur* is fixed rubble and *geröllflur* sliding rubble, or screes. The loose, broken fragments of rock ground off by glaciers from mountain slopes and tops form the lateral, median and terminal *moraines*, and although moraines and screes are not the same, yet the character of the rock debris of which they are composed may be identical, and as far as providing habitats for plants almost exactly similar. The above etymologies have been given in order to clarify our ideas, so that the ecologist may have definite concepts of what scree and talus really are, as providing habitats for single species of plants, or definite associations of species.

European botanists and ecologists, who have been forced to a greater refinement in their studies of vegetation, have led in their study of the plant life of scree, or talus. Little attention has been paid this investigation in America, although there are abundant opportunities in all parts of North and South America for detailed instrumental research to discover the exact ecological relations of the clitter plants. The description of the field observations that follow is a contribution to that end.

Summary of European Investigations

Much of interest to the ecologist, who desires to investigate the scree plants, will be found in a work of Dr. Max Oettli (1905) entitled "Beiträge zur Ökologie der Felsflors Untersuchungen aus dem Curfirsten und Sentisgebiet." In it will be found a general discussion of the rock flora of the region investigated by Oettli with reference to its geographical position, petrography, climatology and the rock plants as they form associations. Three pages are devoted to the plants of talus slopes. The second part of Oettli's monograph deals with the autecology of rock plants.

Rübel (1912) in his presentation of the plant formations of the Bernina region devotes considerable space (pages 203–217) to the vegetation of the Swiss screes in the high mountains of Switzerland. He gives the ecological groups of the talus

plants, as recognized by Schroeter, and emphasizes the fact that climatically the scree plants are confined to the subnival and nival belts of the mountains. He enumerates the plants that are associated with the screes formed from primitive rocks (urgestein) and those that arise from calcareous strata, and in closing his section on scree vegetation the plants found growing on morainic deposits are mentioned.

Two pages of Dr. K. Amberg's "Die Pilatus in seinen pflanzengeographische und wirtschaftlichen Verhältnissen" (1916) comprise a description of the subalpine talus plants, where the growth-forms of the species are discussed with reference to the environmental conditions under which they are found.

The third edition of Dr. Eug. Warming's "Lehrbuch der ökologischen Pflanzengeographie" (1918) gives a brief statement of the conditions under which scree plants grow (pages 745–746). "Œcology of Plants and Introduction to the Study of Plant Communities" as a translation by Percy Groom and Isaac Bayley Balfour of "Plantesamfund" by Eug. Warming and Martin Vahl gives in the second impression (1925) a short account of the formations on rubble and shingles (pages 246–247). Emphasis is laid on the fact that on talus two kinds of formations occur, viz. lithophytes on the stones, and the plants growing between the loose rocks.

Dr. Joh Bär, custodian of the Botanical Museum of the University of Zurich, devotes several pages to a discussion of scree plants with a list of characteristic plants of rocky talus slopes in "Die Vegetation des Val Onsernone" (1918), and so does Dr. August Roth in his "Die Vegetation des Walenseegebietes."

The seventh and eighth chapters (a total of 85 pages) of "Die Pflanzengesellschaften des Lauterbrunnentales und ihre Sukzession" by Dr. Werner Lüdi (1921) comprise a detailed account of the plant life of the talus slopes, which form such a marked feature of the Lauterbrunnen Valley walled in by steep precipices, which have contributed to the formation of extensive screes on both sides of the canyon. The lists and

successional diagrams are useful adjuncts which Dr. Lüdi has used in the interpretation of his research. One of the colored maps of this monograph shows how extensive the talus deposits are in this region of Switzerland.

The successional history of scree vegetation of the Grimsel region is presented in the eighth chapter of "Die Vegetationsverhältnisse der Grimselgegend in Gebiet der zukünftigen Stauseen ein Beitrag zur Kenntnis der Besiedlungsweise von Kalkarmen Silikatfels und Silikatschuttböden" (1922) by Dr. Eduard Frey of Berne, Switzerland. Diagrams of plant successions and plant lists accompany this chapter.

Dr. Ernst Furrer in "Kleine Pflanzengeographie der Schweiz" 1923 makes scanty mention (pages 219–221) of scree vegetation. References will be found throughout the monograph of Dr. Emil Schmid "Vegetationsstudien in den Urner Reusstälern" (1923) to the associations of talus slopes.

The most detailed presentation of the petrography of the talus slopes, the external conditions which influence the growth of talus plants and the several growth forms of the plants found on the screes has been made by Hess and Schroeter. Schroeter in the second edition of "Das Pflanzenleben der Alpen eine Schilderung der Hochgebirgsflora" has given an inclusive account of the investigations of himself, of Hess and others, and this summary will be concluded by giving in outline the statements found in Schroeter's cyclopedic work (pages 667–739). He distinguishes as to the physical character of the talus habitats rocky blocks (over 25 cm. in diameter), coarse stones (25 cm. to 2 cm. in diameter), small stones, or rock fragments (2 cm. to 2 mm. in diameter), sand and clay. These elements are used in a geomorphological discussion of the environment. Then follows an arrangement of the plants of the rock formations and a description of their root systems as related to the substrata. The life conditions of scree plants are as follows: (1) The soil in which the roots grow is covered with a layer of loose rocks with large air spaces between. This conduces to the conservation of soil moisture (the principle of dry farming). (2) The root-holding

soil is not continuous, but separated by rocks into pockets. (3) The talus habitats are not dry, for the water, which is derived from melting snow and rain, is retained owing to the peculiar physical condition of the screes. Longer roots and underground stems are the result of the widely spaced soil pockets into which such parts grow. (4) The changeability of the talus material is one of its marked environmental features. Such changes are due to snow, rain, avalanches, earth slipping, etc. (5) The character of the talus on which the growth of plants depends is influenced by altitude, exposition and rockfall. (6) These alterations in the constitution of the scree materials are followed by a change in the plant associations, in other words, they result in definite floral successions. Schroeter distinguishes five different kinds of ecological growth-forms of talus-inhabitating plants. They are: (1) The scree-wanderers (Schuttwanderer) with elongated, horizontal shoots which grow through the stony materials; (2) The scree-creepers or trailers (Schuttüberkriecher), which grow over the loose rocks by means of rootless stems with apical growths; (3) The scree-stretchers (Schuttstrecker) by means of elongated upright sprouts; (4) The scree-coverers (Schuttdecker) which form rooted mats, or flattened espalier growths; (5) The scree-dammers, or -bankers (Schuttstauer), which form clusters with closely clumped stem bases and roots as in some grasses and sedges. Schroeter gives a detailed account of each type with examples of each.

The classification of Quarles Van Ufford may be displayed as related to that of Schroeter.

1. Lithophiles migrateurs	—Schuttwanderer.
2. Lithophiles recouvreurs	—Schuttüberkriecher Schuttdecker
3. Lithophiles ascendants	—Schuttstrecker Schuttstauer

Scree Plants of Western North America

Screes or talus slopes of loose rocks and rock fragments are found on the mountains of eastern and western North

America. They are found most typically developed in the alpine and sub-alpine belts. The plant life found associated with these rubble heaps varies from north to south, and east to west and some competent botanist should study such vegetation in all of its phases. The ecological investigation of American scree plants would repay richly some ecologist, for it has its practical aspects in the use of these plants in the formation of rock gardens and moraine gardens. There is abundant evidence that American plants succeed better than foreign plants under American conditions of plant environment and the time is opportune with the increased interest in gardens and garden lore to learn which American rock plants are suitable for culture and which can replace the foreign ones, which have been grown largely heretofore. The following notes, although dealing with four mountain ranges, are presented as an introduction to such an investigation of American rock and scree plants. They are based on plants collected by the writer. No collection and study of the rock inhabiting algæ, lichens and mosses were made on the western mountains visited. Where possible, the flowering plants are referred to the five types of growth forms established by Schroeter.

The following account is based on the personal collections and observations of the writer made during the months of July and August, 1926, in Glacier National Park, on the upper slopes of Mt. Rainier and the Canadian Rocky mountains. Supplementary observations are made on several plants collected on Alta Peak at 11,000 feet in the Sierra Nevada mountains of California. Arranged geographically and systematically the following were collected. They were named by Dr. Paul C. Standley and Dr. Willis L. Jepson to whom heartiest thanks are publicly made.

Sierra Nevada Mountain Plants on Alta Peak at 11,000 feet on Aug., 15, 1925. Names by Dr. Willis L. Jepson.

Arabis platysperma Gray (new form of)
Calyptridium umbellatum (Torr.) Greene
Eriogonum incanum (Torr. & Gray (reduced alpine form))

Glacier National Park at Many Glaciers, July 2, 1926 with exception of the starred specimens which were collected on the alpine slopes above Iceberg Lake, July 4, 1925. Named by Dr. Paul C. Standley.

Juniperus siberica Burgersd.

Zygadenus elegans Pursh.
Anemone globosa Nutt.

Rosa acicularis Lind.
**Hedysarum sulphurescens* Rydb.
Rosa Bourgeaniana Crep.
Lupinus argentens Pursh.
Oxytropis splendens Dongl.
Arctostaphylos uva-ursi (L.) Spreng.
Phacelia leucophylla Torr.
**Phacelia Lyallii* (A. Gray) Rydb.
Pentstemon Lyallii A. Gray.
Monarda mentaefolia Benth.
Pentstemon Wilcoxii Rydb.
Aster meritus A. Nels.
Senecio Purshiana Nutt.

Mount Rainier July 9 and 10, 1926. Named by Dr. Paul C. Standley.

Lupinus Lyallii Gray Middle Slopes at 7,000 feet
Lutkea (*Eriogynia*) *pectinata* (Pursh) Ktze.
Saxifraga Tolmiei Torr. & Gray at 6,100 feet.

Canadian Rocky Mountains at Lake Agnes, August 24, 1926. Named by Dr. Paul C. Standley.

Cryptogramma acrostichoides R. Br.
Dryas octopetala Linn.
Fragaria glauca (Wats.) Rydb.
Pentstemon fruticosus (Pursh) Greene.

Growth Forms of the Scree Plants of Western North America

It has been found difficult in the absence of sketches and notes made upon the scree plants in the field to classify them according to the growth forms of Schroeter by the photographs and dried specimens alone. Unfamiliarity with the specific identity of the scree plants, while at work with them in the field, has been found a drawback in their study. The botanist,

or ecologist, who attempts to study in a detailed manner the vegetation of Glacier National Park or Mt. Rainier, or in fact any of the western mountains should spend at least one season in acquiring a familiarity with the native plants and the general conditions under which they grow in preparation for more detailed investigation. Although the writer did not adopt this plan in the study of the florulæ of the screes in western North America, yet these notes are given to direct attention to an important, but neglected, field of botanical and ecological research with the hope that some scientist more fortunately located will be able to give in the future a more detailed report.

1. *Scree-wanderers* (*Schuttwanderer*). According to Schroeter, the scree-wanderers develop from the crown of the roots numerous long shoots which grow between the rocks and form here and there tufts of leaves and flowers which grow up into the light, and if covered by a landslip are able by their growth to reach the surface again. Apparently to this group belong two plants collected by the writer.

Fragaria glauca (Wats.) Rydb., collected on the screes above Lake Agnes in the Canadian Rocky mountains has a deeply penetrating primary root at the summit of which a crown of pentafoliate compound leaves develops. The leaflets are rather thick, 3 to 5 centimeters long, coarsely toothed, silky hairy beneath. It is a perennial herb with long runners (Fig. 1) which here and there root in the rock crevices and form small tufts of leaves. Morphologically related to the strawberry, which produces runners, it is well adapted to the talus habitat.

Lutkea (*Eriogynia*) *pectinata* (Pursh) Ktze. has decumbent or creeping stems which form tufts or mats and leaves at intervals, where the soil pockets between the rocks permit such a development to take place. The small leaves are biternately parted into linear lobes with the flowers. The plant studied by the writer was collected on the rocky upper slopes of Paradise Valley, Mt. Rainier. *Lutkea* is a plant usually found on the high mountain slopes near per-

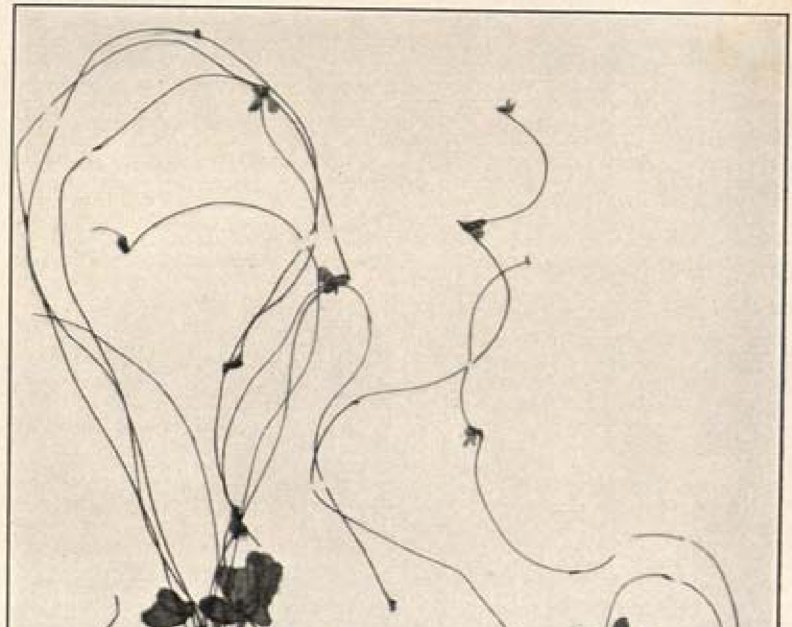

FIG. 1.—*Fragaria glauca* (Wats.) Rydb., a scree-wanderer.

petual snow and is distributed from Mt. Shasta, California north to Alaska.

2. *Scree-creepers or Scree-trailers* (*Schuttüberkriecher*). The plants of this group have loose or slack superficial shoots (oberirdischen Trieben) without roots which grow by means of apical stem growth. Usually the shoots are prostrate on the surface of the loose rocks, or occasionally they grow between them. Apparently seven species collected by me belong to this category.

Juniperus siberica Burgsd. The ground juniper is a prostrate, or spreading shrub sometimes a meter high, but usually forming broad clumps, or carpets with sharp-pointed leaves twisted at the base. Standley in his "Flora of Glacier Park, Montana" adds to his description of the shrub this statement: "Perhaps only a form of *J. communis* L. On exposed slopes at either high or low elevations, the plants are often prostrate and form extensive slippery mats over which it is difficult to climb. Ground juniper frequently grows with the creeping cedar." The specimen studied was collected on the screes at Many Glaciers, Glacier National Park.

Rosa acicularis Lindl. This rose grows on the scree in Glacier National Park and forms brush growth not over a meter high with spreading flat branches that hug the rocky slopes on which it occurs.

Rosa Bourgeaniana Crep. The description given for the preceding rose applies to this rose, which is probably the most common specimen in Glacier National Park.

Hedysarum sulphurescens Rydb. The plants of this species of *Hedysarum* with yellow flowers grow usually 30 to 50 centimeters high, but above timber line, the plants are frequently only 10 centimeters tall. It has a sprawling habit of growth, which places it among the scree-trailers.

Lupinus argenteus Pursh. There is no character except the crooked stems in the herbarium specimens to indicate the growth form of this species of lupine, which according to Standley is a branched perennial 40 to 80 centimeters high. Tentatively it has been placed in the second group of scree plants.

Pentstemon fruticosus (Pursh) Greene. This decumbent, shrubby species of beard-tongue grows 2–4 decimeters tall. The specimens collected on the screes above Lake Agnes have branches about 2–3 millimeters thick, which bend downward and then curve upward to the leafy, flower- and fruit-bearing shoots (Fig. 2). It clearly belongs to the scree-trailers, and so does *Pentstemon Wilcoxii* Rydb. collected on the screes at Many Glaciers, Glacier National Park, Montana on July 2, 1926.

3. *Scree-stretchers* (*Schuttstrecker*). The scree plants of this group develop a few upright shoots which work up through the rocks and stones, in other words they stretch up to the surface. The following five plants belong to this class.

Zygadenus elegans Pursh. The bulbs of this monocotyledonous herb are hidden usually beyond reach in some rock cranny, or cavity. The linear leaves and scapes reach the surface where they form clumps with the raceme of greenish-white or pale greenish flowers in evidence in Glacier National Park on July 2, 1926, when specimens were collected.

Anemone globosa Nutt. The common western anemone is a perennial plant with buried rootstalks from which a few basal, deeply dissected leaves arise. It was found on rock slides in Glacier National Park. Its hairy, one-flowered, fruiting stem was 20 centimeters tall.

Lupinus Lyallii Gray was found among loose rocks on Mt. Rainier at 7,000 feet in full flower on July 10, 1926. It formed dense clumps of stems arising from a deeply penetrating primary root. In form it is a scree-stretcher, although with large plants it might become a scree-coverer and belong to the next group. Its palmately compound leaves with 7 or more small leaflets are silky with oppressed, silvery hairs. Its blue-purple flowers are in abundance on a single plant.

Oxytropis splendens Dougl. This purple-flowered loco weed is covered with long, loose, silky hairs. Its raceme-bearing scopes arise from a vertical root stock descending between the loose rocks of the screes. The basal, or radical, pinnately compound leaves are silky and shorter than the scope (Fig. 3). It is a true scree-stretcher.

Fig. 2.—*Pentstemon fruticosus* Rydb., a scree-creeper or a scree-trailer.

FIG. 3.—*Oxytropis splendens* Dougl., a scree-stretcher.

FIG. 4.—*Saxifraga Tolmiei* Torr. & Gray, a scree-coverer.

FIG. 5.—*Phacelia Lyallii* A. Gray, a scree-coverer.

FIG. 6.—*Phacelia Lyallii* A. Gray, a scree-coverer.

Penstemon Lyallii A. Gray is one of the most showy scree plants of the third group in Glacier National Park with its pale-purple corollas 3 to 4 centimeters long. Its stems are usually found upright in dense clumps.

4. *Scree-coverers* (*Schuttdecker*). The plants of this class form broadly spreading shoots. The spreading branches are rooted beneath and from them arises short, upright, leafy and flower-bearing shoots. Three of the scree plants of western North America belong here.

Dryas octopetala Linn. This species of alpine avens collected on the screes above Lake Agnes in the Canadian Rocky mountains on August 24, 1926, is placed definitely among the scree-coverers by Dr. Schroeter, as it is a plant of the European Alps also. It forms in America, as in Europe, dense, low mats which cover the screes as with a Persian carpet.

Saxifraga Tolmiei Torr. & Gray. This saxifrage grew on the rock debris on the slopes of Mt. Rainier at 6,000 feet and was collected there on July 9, 1926 (Fig. 4). Its stems are about the thickness of cords. Its leaves are small, obovate and leathery and the flowers are star-like.

Phacelia Lyallii A. Gray was abundant on the screes above Iceberg Lake, Glacier National Park, where it was collected on July 11, 1926. Its stems covered with brown scale, leaves grow flat on the ground and the leafy shoots with pinnately cut leaves are formed in sufficient numbers to form densely crowded shoots. The short crowded racemes of blue flowers arise in numbers from the clumped erect shoots. Figure 5 shows the environmental conditions under which the plant grows and Plate 50-B in Standley's "Flora of Glacier National Park, Montana" a flowering plant half natural size. A reproduced photograph of the plant is shown in Figure 6.

5. *Scree-dammers, or Scree-bankers* (*Schuttstauer*). The scree-dammers, or scree-bankers are plants with their roots and shoots in hassocks, or tussocks (*Horsten*) of greater or smaller size. Where found on the talus slopes, or rock slides, they stabilize the screes very much like stakes driven into a

grassy bank to prevent the sods of grass from slipping down. One plant of this group was collected by the writer.

Cryptogramma acrostichoides R. Br. The parsley fern has a tufted habit with fertile fronds taller than the sterile. The numerous stipes are straw colored. This common fern grows abundantly on the screes above Lake Agnes in the Canadian Rocky mountains and was collected there (Fig. 7). It is a typical scree-banker.

Addenda

A few notes should be added about a few scree plants of Alta Peak, 11,000 feet in altitude, one of the high Sierra Nevada mountains back of Giant Forest in Sequoia National Park. Out of the rubble on the top of this peak grew *Eriogonum incanum* Torr. & Gray in a prostrate, reduced alpine form in full flower on August 15, 1926. It is a scree-creeper, or scree-trailer. Its leaves are canescent and the plant arises from a long, deep-seated primary root producing secondary roots at its lower extremity. *Calyptridium umbellatum* (Torr.) Greene was collected near the summit of Alta Peak at 11,000 feet. With rosettes of spatulate leaves and flowers in pedunculate heads, it belongs to the group of scree-stretchers, as does likewise *Arabis platysperma* Gray with long primary root, clusters of narrow leaves and numerous pedunculate siliques, as it is a cruciferous plant. It was gathered at 10,500 feet among the loose rocks of Alta Peak.

Conclusion

It should be mentioned in closing that Standley in his "Flora of Glacier National Park, Montana" gives a number of illustrations of typical scree plants. They are as follows: Plate 44-B illustrates an arctic-alpine rock slide. Plate 48-B is a photographic reproduction of *Dryas octopetala* on rocks. Plate 50-A shows the dwarf form of the red raspberry (*Rubus strigosus*) on rock slides, and Plate 55-B the habit of the blue phacelia (*Phacelia Lyallii*).

FIG. 7.—*Cryptogramma acrostichoides* R. Br., a scree-dammer.

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SOME OBSERVATIONS ON COMPARATIVE CONSTITUTION IN MAN AND THE LOWER MAMMALS

By HERBERT FOX

(*Read March 1, 1929*)

THE invitation by the President to address the Philosophical Society on some subject in the work of the Philadelphia Zoölogical Society and the Department of Comparative Pathology at the University was received with deep appreciation of this privilege. Hesitation was at first felt because our field is so largely technical that it appeals more directly to the specialist in medicine and its immediately related sciences, but I was emboldened to accept by the observation in the Introduction of Sir John Bland-Sutton's "Evolution and Disease" that pathology is one phase of general biology, perhaps gone astray, perhaps obeying some law of evolution, but at all events of interest to students of the larger field. And so in the following pages will be found some observations on the morbid changes discovered in the animals of the Garden, studied from the viewpoint of constitution, a subject that is being more and more closely investigated in the newer thought in biometrics.

As one reviews human medical history, it is noted that the ancients, having no adequate dissections or pathological bases, represented the observational period when persons were divided according to appearances, as plethoric, apoplectic, phthisic and the like, while Hippocrates developed the combinations of hot and cold, dry and moist. Much of this persisted through the scholastic dark and middle ages until we come to the time of Harvey, whose methods might be stated as initiating the organic experimental period. At all events his philosophy that anatomy was a guide to function, directed thought away from the body as a whole to its several parts.

This "part-specialism" has continued to be more and more detailed until now we split genes. Early in the nineteenth century Laycock and then Jonathan Hutchinson did much work on clinical types of individual disease in man, and later in this period came Lombroso, at work on the whole body again, with the result that he established certain psychological types of more or less acceptability. His influence was felt by other Italians, which nation has been followed by Germany in a vigorous study of constitutional biometrics of man. The past twenty years have seen many important contributions to the subject from American students (Hrdlička, Goldthwaite and others). Accurate measurements are taking the place of visual impressions, or word pictures, in describing types of individuals that show certain anatomical, physiological or psychological tendencies.

There is no doubt that the study of the ductless glands, so very actively driven in the past two score years, has greatly influenced the investigation into constitution because it is accepted today that the balance of endocrine or hormonic action in the body is responsible for its optimum development, whereas imbalance, of even one unit of the system leaves a distinct imprint on the individual. If this effect on ontogeny be true, it is possible that phylogenic evolution may be influenced, probably more slowly, but yet with evidence that later we may learn to recognize.

Constitution is a state or condition of construction that has, according to Draper, four "panels of personality"—anatomy, physiology, psychology and immunity. While there may be measureable elements or features in the makeup of any given zoölogical unit or individual, the product is due to relationship that these separable elements bear to one another and perhaps to the completed whole. Draper goes on to define constitution as an aggregate of hereditarial characters, influenced more or less by environment, which determines the individual's reaction, successful or unsuccessful, to the stress of environment. Such a conception is equivalent to making a sum of hereditary characters, heredity

being the chief factor, working against environment as an incidental or recording medium. Perhaps this is Spencerian orthodoxy, but it ascribes to environment much less weight than most modern biologists are willing to accept. However, it applies in the material that has been studied in our laboratory to be presented shortly, since there is a purity of racial and phylogenic strain in our material lacking in the human material studied by anthropologists. To what extent constitution, with its organic morphologic panels or elements, may effect evolution, is certainly an open field for research.

The idea that constitution is a mere sum of the various units that make up a being has found favor in Germany in the writings of Krause, Martius and Bauer, while the Italian school credits to environment and to interbalance of anatomy, physiology and psychology a much greater weight. They prefer rather to call the subject "biotypology." They do, however, apparently believe that germinal cells at the moment of conception may be influenced by conditions, exogenous to the body at the moment of union, almost as definitely as they have been previously modified by endogenous stimuli prior to conception or could be affected later during gestation. That such is the case seems a matter of pure speculation.

Pende's definition of constitution is needlessly cumbersome. "The constitution is the morphological, physiological and psychological resultant (variable in each individual) of the properties of all the cellular and humoral elements of the body, and of the combination of these in a special cellular state having a balance and functional output of its own, a given capacity for adaptation and a mode of reaction to its environmental stimuli. Such a resultant is determined primarily by the laws of heredity and secondarily by the disturbing influences exercised by the environment upon the individual's hereditary plan of organization." I have ventured to shorten it to the following: Constitution is qualitative, anatomic, physiologic and psychologic individualism, the result of environment upon hereditary predisposition. This would permit of variations of balance between the major organic and

functional systems of the body, yet provide for their behavior during the developmental periods. I shall take liberties with the word "individualism" for it will be found applied to groups of animals much more than to one example; it has already been stated that greater purity exists in lower mammalian groups than in man. This definition provides also for prominence or predominance of one section of the whole, as for example, anatomical over psychological evolution, as already a century ago noted in the following words by Rostan: "It is rare for a perfect balance to exist in all the systems of the animal economy. This complete harmony has perhaps never existed outside of the imagination of the ancients. One system nearly always appears to predominate over all the rest. Accordingly it is easy to conceive that the dominance of one apparatus should imprint an important modification upon our physical and moral constitution." Certain it is that in the zoölogical material with which we are concerned, dominant panels are copiously represented, so plainly indeed that they must be evaluated in judging pathologic deviations. Thus, for example, there are uniparous and pluriparous animals, there are constant plantigrade and partial orthograde varieties, there are variations in longevity, there are hibernating and non-hibernating groups, there are specialized mechanisms for the perpetuating of species, most animals have a nutritional, that is, gastrointestinal dominant, and in all the psychic element bears a definitely lower relative value than in man, but nevertheless has almost certainly an ontogenetic value as recent work of animal psychologists suggests.

A further explanation of constitution has been attempted by Viola and Pende in a diagram (Constitutional Inadequacies, Phila. 1928, p. 83). It illustrates Pende's definition as well as could be done, but it leaves, as all such diagrammatic figures do, the matter of relativity of each segment to others entirely out of the evaluation, each side of the pyramid being the same. We are concerned with the left triangle chiefly, but we can adduce a few facts concerning the hormonic part of the basal face.

Before passing to a description of our material and methods of study, an additional word might be added here upon the importance of the glands with an internal secretion, those structures that discharge their hormones—"postal messengers," Keith calls them—into the blood and lymph and not by specialized ducts. The thyroid, thymus, pituitary, adrenal, sex glands are the most important and concern us here, but there are others of less significance according to modern knowledge. Many writers, most persuasively and most elegantly Keith, would see optimum development of the body as an expression of perfect balance of the functions of these organs and would ascribe to their imbalance the occurrence of deviation of all kinds. It has been stated that age expectancy is a function of endocrine adjustment. If pathology be a guide to this end, we may find some indirect confirmation. It is not the place to discuss all the functions of these glands, but with the notes upon abnormalities will be found their relation to certain normal and morbid changes.

Anthropological study of constitution has but a single animal type although many racial influences are blended. In a study of constitution of lower mammals there are many groups of widely different habitat, reproduction, habits and morphological construction. It is therefore impossible to make close comparisons with man, and it shall have to suffice to point out what disease follows what natural features, leaving to chance a few analogies, there being no strict homologies. It is of course possible to make accurate measurements of dead animals, but this has been done in so fragmentary a manner that conclusions are no better than gross visual records would justify. It will be seen that the features and criteria that we have used are easily separable and can be repeated and perhaps improved upon by all scientific workers.

It may be protested that the material upon which this report is based was collected from captive animals that would not reveal the responses obtainable under natural conditions. Until appropriate data are dependably collected by naturalists in the field, we shall be obliged to rely upon figures obtained

in menageries. The effect of our records is to reveal just what system breaks down, what organ is the seat of the lesion that led to the animal's end, what tissue yields to morbid agencies. This is essentially Pearl's reasoning in establishing organic susceptibility. We are aware that captive animals do not obtain the food that they would select, and we admit that in some cases the food they have received has not been correct, for one or two diseases have been modified by alteration of diet. The absence of chase and of proper mating almost certainly modifies to some extent the morbid responses. All that we can do is to report the reactions as they appear, with the most careful scrutiny of known modifying influences, use caution in drawing conclusions, and never forget that man wilfully modifies his responses by inappropriate food, clothing and habits and by vices.

The data that have been mustered here are taken from our continuous filing system of zoölogical orders and pathological diagnoses. The studies and results are from our own material only since publications from no other laboratories are reported in a manner permitting analytical computations. Indeed from the London Society alone are dependable data published.

There have been selected for study of constitution the five mammalian orders from which we have the greatest number of examples—Primates, Carnivora, Rodentia, Ungulata, and Marsupialia. These have been divided into their constituent families and listed with the diagnoses of eight of the most important systems. A few of the families are omitted from our total because too few examples of unimportant characters were found, or they failed to show at autopsy the lesions covered in this work. Thus, for example, the Hippopotamidæ are not discussed because only four recently born bodies have been available for study. Inclusion of such material might give false percentages. So far as possible, only grown, mature, fertile specimens were included, but no assurance can be given since we have usually inadequate knowledge of the age of a specimen.

In all there are 2,571 representatives of 37 families of 5

orders, varying in separate examples from 2 giraffes to 429 Old World monkeys. The morbid conditions selected for study were chosen because they represent the most important organs in *human* morbidity and mortality.

The heart and blood vessels, commonly considered together by vital statisticians, are responsible for the majority of adult human deaths, and are closely followed by the kidneys (Bright's disease) as seats of morbid changes. The respiratory tract, with its outstanding disease, pneumonia, supplies one th[illegible] the causes of death at all ages during the winter epidemic periods. The gastrointestinal tract is much less important for the human adult but a frequent site of disease in the young, and a very vulnerable spot in both wild and domesticated animals. Tuberculosis, the seventh most frequent cause of human death, is selected as an example of susceptibility to chronic infectious disease. The skeleton is an indicator of growth and appropriateness of housing and feeding conditions in both human and wild animals, and since disease in it is frequently associated with alterations in the ductless glands, both systems were analyzed. One cannot learn too much of tumors and they were included for their association with specific family characters and susceptibilities.

Here follow charts, tables and illustrations of the orders, families and examples of each, and showing the percentages that each group suffered with each disease selected. Table No. II is perhaps the more interesting for it illustrates to what extent each family participated in the total incidence of each disease in the total for the Mammalia studied. This will be discussed in detail as each character is analyzed.

I—Analysis of Morbid Reactions in Zoölogical Orders.

The Primates.—This order, exclusive of man, has apparently two very vulnerable points, its resistance to tuberculosis and its intestinal tract. Tuberculosis in the lower Primates is not so conspicuously a disease of the lungs as it is in man. The abdominal organs are frequently affected to the highest degree and the lungs may be practically normal. Reason for

CHART I

PERCENTAGE OF ANIMALS PER FAMILY SHOWING LESIONS

	No.	H. & B.V.	Kid.	Resp.	G.-I.	Tbc.	Ductl. Glds.	Skel.	Tumors
PRIMATES									
Simiidæ	15	6.6	20.	20.	46.	26.			
Cercopithecidæ	429	4.4	8.6	10.	19.1	42.	.9	5.8	.7
Cebidæ	160	5.	9.3	5.6	22.5	14.4	1.7	30.	.7
Hapalidæ	62	1.6	11.	14.5	24.			21.	
CARNIVORA									
Felidæ	176	5.7	10.8	11.4	58.5	2.3	5.1	.6	4.5
Viverridæ	42	14.3	19.	12.	31.	12.			7.1
Hynæidæ	5			20.	40.		80.		20.
Canidæ	171	12.8	15.2	8.2	43.8	1.1	20.	1.7	6.4
Mustelidæ	117	6.	21.3	14.5	33.3	.8	7.7	.8	
Procyonidæ	116	3.4	18.1	13.7	20.7	2.5	12.1	6.	3.4
Ursidæ	56	7.2	16.	2.	70.	3.6	5.	1.8	7.1
Otariidæ	32	15.6	15.6	34.4	40.6				
RODENTIA									
Sciuridæ	91		14.3	10.	17.6			14.3	5.5
Castoridæ	21	9.5	14.3	14.3	43.	9.5			
Muridæ	37		18.9	2.7	13.5			2.7	21.6
Dipodidæ	3		33.		33.				
Heteromyidæ	12			8.5	16.6			8.5	16.6
Octodontidæ	16		18.8	6.2	18.8		6.2		25.
Hystricidæ	69	11.6	11.6	14.5	34.8		1.4	5.8	2.9
Chinchillidæ	5			20.	40.				
Dasyproctidæ	26		7.7	7.7	15.4	3.9		3.9	7.7
Caviidæ	17	17.6	11.7	5.8	58.8	5.8			
UNGULATA									
Tapiridæ	16	12.5	31.2	6.2	68.7	12.5			
Equidæ	21	9.5	9.5	9.5	47.6	4.8			4.8
Bovidæ	215	10.7	13.5	7.9	16.3	8.8	.9	.45	2.7
Cervidæ	251	5.6	6.8	5.6	16.3	12.	.4		.8
Antilocapridæ	6	16.	16.	16.	16.	16.	16.		
Giraffidæ	2				100.				
Camelidæ	37	21.6	10.8	16.2	13.5	5.4	5.4		2.7
Suidæ	21	4.7	19.	14.3	38.1		4.7		14.7
Tayassuidæ	10	10.	20.	10.	60.				
MARSUPIALIA									
Didelphyidæ	167	10.1	10.1	10.1	9.9	1.2	3.6	18.2	3.
Dasyuridæ	14	21.4	46.6	13.3	40.		21.		20.
Parameliidæ	6	16.6	33.	33.	33.				16.6
Phascolomyidæ	4			50.	50.				
Phalangeridæ	11	9.		9.	27.				
Macropodidæ	112	7.1	3.5	11.6	13.3		1.8	.9	1.8

PRIMATES

Simiidæ .... Anthropoid Apes (gorillas, chimpanzees, orang-utans, gibbons).
Cercopithecidæ . Old World Monkeys (macaques, baboons).
Cebidæ .... New World Monkeys (capucins, howlers, spiders).
Hapalidæ....... New World Monkeys (marmosets).

this is partly to be found in the habit of the animal of taking much into their mouths, but this cannot be all, since the anatomy of the primate gastrointestinal and pulmonary systems is nearly identic throughout, so that it seems necessary to assume a greater receptivity on the part of the abdominal organs or a greater resistance by the lungs. Whether or not the greater motility of the heart and its dextro-position in the monkey can have an influence by allowing directer drive to the circulation and better blood supply to the lungs, can be a speculation.

The gastrointestinal disease incidence of the primate may be due to their habits, for there is no anatomic reason in the mucous membrane to account for it, and stasis of intestinal contents does not occur. Another feature of primate pathology is the high incidence of Bright's disease in the form nearest man, the Simiidæ. Just why this is, is not clear, because they do not suffer from affections that are held responsible for its causation. This fact shows a 'renal panel' of the apes.

The New World monkey is most susceptible to disease of the bones. This is perhaps due largely to inappropriate diet, but I have observed that the periosteum of these animals is definitely more delicate than it is in the Old World monkeys and in man. While major bone growth is at the ends, the maintenance of tonus is left to the periosteum. Another feature that may be of importance in the skeletal susceptibility of this group and of the Hapalidæ (Marmosets) is that the intestinal tract is smaller and more delicate than is this system in the higher Primates.

While some families of Primates are equipped with heavy canine teeth, they are almost exclusively herbivorous. I am not at all sure they do not eat small animals when in their native habitat and they will eat small portions of beef stew in captivity. It is noteworthy that their diseases resemble those of one of the other two herbivorous groups, the Ungulata, which have a very different dental and gastric anatomy.

Carnivora

Felidæ	Cats.	
Viverridæ	Civets, Genets, Paradoxures, Ichneumons,	
Hyænidæ	Hyena.	
Canidæ	Dogs, Wolves, Foxes, Jackalls, etc.	
Mustelidæ	Marten, Skunk, Weasel, Otter, Badger, etc.	
Procyonidæ	Raccoon, Bassaris, Coati, Kinkajou.	
Ursidæ	Bear.	
Otariidæ	Eared Seal, Sea Lion	These are grouped separately as sub-order Pinnipedia illustrating water carnivores.
Phocidæ	Common Seal Walrus	

The Carnivora.—This group, with its powerful jaws, rapid deglutition, strongly muscular intestines and short digestive tube, has an outstanding susceptibility of the intestinal tract. One half of these animals suffered with disease in these tissues. And it is striking that the bears, the least carnivorous of the lot, had the highest incidence, an apparently definite evidence of organic susceptibility. Coupled with this tendency, it can be found by further analysis, is a distinct association of kidney disease and to a less extent, cardiac disease. The respiratory tract is apparently quite resistant and this I venture to ascribe to the width and large number of bronchi and to the richness of blood supply characteristic of the lungs of these animals. Dogs, hyænas and raccoons have very susceptible thyroid glands. The whole order is very susceptible to tumors of all kinds but those in the thyroid, a ductless gland, head the list. Attention is directed to the great susceptibility of the lungs of sea lions to inflammatory disease.

Rodentia

Sciuridæ	Squirrels, Spermophiles, Marmots.
Castoridæ	Beaver.
Muridæ	Rats, Mice.
Geomyidæ	Pouched Rats, "Gophers."
Dipodidæ	Jumping Mice, Jerboas.
Heteromyidæ	Kangaroo Rats.
Octodontidæ	Capromys, Coypu.
Hystricidæ	Porcupines.
Chinchillidæ	Viscacha, Chinchilla.
Dasyproctidæ	Agouti, Spotted Cavy.
Caviidæ	Guinea Pig, Capybara.
Leporidæ	Rabbits, Hare.

Rodentia.—Perhaps the most striking figure in the incidence of disease in this group is the freedom from heart and blood vessel lesions, only two families, beavers and porcupines, being represented. The kidney, however, is often the seat of chronic Bright's disease. What adds to unusualness of the cardiac response is that, according to our weight records, the rodent has the smallest heart in proportion to body weight among the mammals here studied.

Rodents are the greatest bearers of tumors of all kinds, especially malignant ones, but do not exhibit so many malignant degenerations of the ductless glands as do the carnivores. This is well known under domestic conditions and has been observed on rodents caught in the wild. It has not been adequately explained and must be considered as a definite morbid deviation or panel. Squirrels alone of the rodents are susceptible to bone disease.

UNGULATA	
PERISSODACTYLA (odd toed)	
Rhinocerotidæ	Rhinoceros.
Tapiridæ	Tapir.
Equidæ	Horse, Ass.
ARTIODACTYLA (even toes).	
Bovidæ	Oxen, Antelopes, Sheep, Goats.
Cervidæ	Deer, Moose, Elk.
Antilocapridæ	Prong-horned Antelope.
Giraffidæ	Giraffe.
Tragulidæ	Chevrotains, Muis Deer.
Camelidæ	Camels, Llama.
Hippopotamidæ	Hippopotamus
Suidæ	Swine, Warthogs.
Tayassuidæ	Peccaries.

Ungulata.—This order comprises families and genera of very widely differing gross characters and physiology. Their herbivorous diet and maxillary construction remain comparable throughout, but in no other zoölogical order do we have so many varieties in the intestinal organs (vide infra). These animals have the most vulnerable heart, vessels and kidneys of all our groups, have a low respiratory vulnerability, a variable intestinal vulnerability, and, next to the Primates,

the most tuberculosis. Despite their macrosomia, the dystrophies or abiotrophies of the ductless glands, so important in human medicine and so often responsible for macrosomia and microsomia, are conspicuous by their infrequency. Nor do tumors and skeletal diseases, except in swine, present high figures.

That the cardiorenal complex is vulnerable should be considered in terms of the following constructional peculiarities. The metabolism of these animals is largely acid in the end products, and there is little alkaline base in the diet to neutralize it. There is constantly great pressure within the abdominal area. The intestinal tract is very large in square area of mucosa, permitting much bacterial and toxic absorption. The heart of these animals is quite free in the thorax, there being relatively less fulcral insertion to the mediastinal tissues, sternum and vertebræ than in other orders except the Marsupialia. The cardiac stalk vessels are large and strong but the branch vessels are small and have relatively delicate walls. The habit of these animals is for them to use sudden rapid straining muscular exertion. They are, in the groups represented here, all macrosomic. The causes to which are usually ascribed heart disease in man are conspicuously lacking in the ungulates.

The gastrointestinal reaction of this order is most interesting. There are two groups according to the gross anatomy of this tract, one comprising the Perissodactyla (odd-toed) and the Suidæ and Tayassuidæ (swine and peccaries) among the Artiodactyla, the other group comprising all the remaining even-toed varieties. The first group has a simple man-like, single stomach and rather simple tubular intestines with sturdy walls. The second group has a compound gastric organ and three separate divisions of the intestinal tube, mostly with delicate walls. The simple stomach and tube animals have more gastrointestinal inflammation than the compound system as 2 is to 1. Thus it would appear that the compound stomach of the ruminant type is a guard for the lower tube and is in itself resistant. It must not be over-

looked in this connection that the animals with the simple stomach eat many vegetables, food not so generously supplied to ruminants, and that these plants are well known to favor the growth of fermentative, putrefactive and pathogenic microbes.

Tuberculosis presents such a definite picture in the lungs of this order that I have described it elsewhere as representing an ordinate peculiarity, no other group reproducing it except in greatest rarity. Anatomically considered, the capacious respiratory tubes, the richness of lymphatic tissue and the relatively low vascular supply of ungulate pulmonary tissues should be recalled as possibly favoring the growth of the tubercle germ. But this is not enough and we shall be obliged to fall back upon the unexplained feature of great susceptibility.

MARSUPIALIA

Didelphyidæ	Opossums.
Dasyuridæ	Dasyures, Tasmanian "Devils."
Peramelidæ	Bandicoots.
Phascolomyidæ	Wombat.
Phalangeridæ	Phalangers.
Macropodidæ	Kangaroos, Wallabies.

Marsupialia.—This group is divisible into three subgroups of two each, the first carnivorous in habit and gastric construction, the second omnivorous in these characters and a third that is strictly herbivorous. The susceptibility of the heart, vessels and kidneys in the carnivorous group may be mentioned. No definite statement can be made about the respiratory tract. Those of mixed diets have the greatest intestinal susceptibility. It seems interesting that the strictest carnivorous marsupial and herbivorous marsupial, the opossum and the kangaroo respectively, should have the most resistant digestive system. The dasyures and devils have a definite tendency to ductless gland disease and to tumors. They do not live especially well in captivity. The high record for bone disease in the opossums is due to several litters of young animals brought to the Garden that failed to thrive in captivity. This probably means a sensitive,

vulnerable bony system and metabolism because they do not have susceptible intestines or ductless glands.

II. Special Distribution of Disease. Chart II

TABLE SHOWING PERCENTAGE OF TOTAL ORGANIC DISEASE CONTRIBUTED BY EACH FAMILY

	Total	H. & B.V.	Kid.	Resp.	G.-I. Tr.	Tbc.	Ductl. Glds.	Skel.	Tumors
Primates									
1. Simiidæ	15	.5	1.	1.1	1.1	1.4			
2. Cercopithecidæ	429	1.	12.	16.	11.9	62.	4.	16.5	3.6
3. Cebidæ	160	4.4	4.8	3.3	5.2	7.8	3.	31.5	1.2
4. Hapalidæ	62	.5	2.2	3.3	2.2			8.5	
Carnivora									
5. Felidæ	176	5.4	6.1	7.3	15.	1.4	9.	.65	9.7
6. Viverridæ	42	3.3	2.6	2.5	1.9	2.4			3.6
7. Hyænidæ	5			.36	.29		4.		1.2
8. Canidæ	171	1.2	8.4	5.1	10.9	.7	34.	1.9	13.4
9. Mustelidæ	117	3.1	8.1	6.2	5.6	.34	9.	.65	
10. Procyonidæ	116	2.2	6.8	5.9	3.4	1.3	14.	4.6	4.8
11. Ursidæ	56	2.2	2.9	4.	5.6	.7	3.	.65	4.8
12. Otariidæ	32	2.7	1.6	4.	1.9				
Rodentia									
13. Sciuridæ	91		4.2	3.3	2.3			8.5	6.1
14. Castoridæ	21	1.1	1.	1.1	1.3	.69			
15. Muridæ	37		2.2	.36	.73			.65	9.7
16. Dipodidæ	3		.32		.14				
17. Heteromyidæ	12			.36	.29			.65	2.4
18. Octodontidæ	16		.1	.36	.43		.1		4.7
19. Hystricidæ	69	4.4	2.6	3.7	3.4		1.	2.6	2.4
20. Chinchillidæ	5			.36	.29				
21. Dasyproctidæ	26		.65	.73	.58	.24		.65	2.4
22. Caviidæ	17	1.6	.65	.36	1.4	.34			
Ungulata									
23. Tapiridæ	16	1.1	1.6	.36	1.5	.69			
24. Equidæ	21	1.1	.65	.73	1.4	.34			1.2
25. Bovidæ	215	12.	9.4	6.2	5.	6.6	2.	.65	7.3
26. Cervidæ	251	7.7	5.5	5.1	5.9	10.3	1.		2.4
27. Antilocapridæ	6	.52	.32	.36	.14	.34	1.		
28. Giraffidæ	2				.29				
29. Camelidæ	37	4.4	1.3	2.2	.73	.69	2.		1.2
30. Suidæ	21	.54	1.3	1.1	1.2		1.		3.6
31. Tayassuidæ	10	.54	.65	.36	.9				
Marsupialia									
32. Didelphyidæ	167	9.3	5.5	6.2	2.3	.69	6.	23.	6.1
33. Dasyuridæ	14	1.6	2.2	.73	.9		3.		3.6
34. Paramelidæ	6	.54	.65	.73	.29				1.2
35. Phascolomyidæ	4			.73	.29				
36. Phalangeridæ	11	.52		.36	.43				
37. Macropodidæ	112	4.4	1.3	4.7	2.2		2.2	.65	2.4

Having discussed the features that characterize the orders in their reaction to disease agencies, we shall now pass to an analysis of the diseases themselves, to learn if possible which

animal groups they select, if I may be allowed this figure of speech. Analysis is made of the first five groups in order of susceptibility.

Heart and blood vessel disease attacks mostly the Bovidæ, large ruminants. In the third place come their nearest zoological relatives, the Cervidæ. Some suggestions to explain this have already been made. Opossums occupy the second position, but it must be explained that this is probably due to a small epizoötic in a group of cages and the figures may be misleading. The heart of the opossum is a relatively large, well balanced organ, hung loose in the anterior mediastinum. It is moderately susceptible to degenerative changes, but the several cases of our list were definite acute inflammations. The fourth and fifth positions are occupied by the Felidæ, Hystricidæ and Cebidæ. Thus we have mammals of all sizes and habits represented, but the leaders of the list are large herbivores. Thus we find the actual leaders in cardiovascular disease among the macrosomic, megalosplanchnic varieties that have a large pendulous heart, vessels of possibly inferior disproportion, relatively small, often non-lobated lungs, and a larger right auriculo-ventricular bulk than most other mammals. This corresponds to a certain degree with the cardiovascular constitution in man described by Pende. The vascular group of Draper, who does not study the heart directly, is found in short stocky males and in tall, heavily made females of the human species.

Kidney.—The New World monkeys—herbivorous, tree-living, with only partly developed canines, a delicate intestinal tube and a compound kidney—have easily the lead in renal disease. They are followed by the Bovidæ, which have such a definite cardiac susceptibility, therefore presenting the picture so frequently the cause of middle age invalidism in man. Wild dogs, skunks and raccoons take the next three places. Again Herbivora lead Carnivora. This grouping is not comparable with anything in human pathology. As in the foregoing section, the heart, Pende and Draper found heavily set individuals, usually with large organs and disproportionately

small vessels, to be most affected by Bright's disease. The bovines alone, occupying the second place, correspond to this description.

Respiratory Tract.—Pneumonia, bronchitis, pleurisy and similar diseases are twice more often found in the South American monkeys than in any other animal. This is probably due to their tropical and semitropical habitat and inability to become acclimated, but it nevertheless shows how the vulnerability of its lungs compares with that of other groups. Even though the cats are not often carried off by pulmonary disease, their percentage of frequency among the whole number of respiratory deaths is second, but not greatly removed from other carnivores. The bovines, deer and opossums likewise have distinct pulmonary vulnerability. On the whole, carnivorous mammals have greater liability to respiratory disease than do herbivores.

A respiratory type in man as described by students of constitution is so modified by explanations of the reason for tuberculosis as located in the apex of the lung, that comparisons and contrasts are difficult. However, most descriptions would permit a typification of a respiratory-vulnerable man as heavy, tall, "large-hearted, small-vesseled." No analogy can be found in our material.

Gastrointestinal Tract.—The first five percentile contributors to the total of disease of this system are in order, Felidæ, Cercopithecidæ, Canidæ, Cervidæ, Mustelidæ with Ursidæ being the same percentage in the fifth place. There is little room for comment as all groups are represented. This only emphasizes the fact, mentioned earlier, that the lower mammals have an intestinal dominant, in this case pathologic. Numerically the cases in the herbivores are greater than in the carnivores, but in percentage the difference is in favor of the meat-eating group. One can only observe that tracts that are unprotected by a compound stomach and are relatively short, hold the first three places, and that they are in mesosomic and mesosplanchnic species. In man there are two illy defined gastrointestinal types, one a tall slender or

dolichomorphic, microsplanchnic person, and the other just the reverse.

Tuberculosis.—This disease, studied because of its economic importance and its value as an evidence of response to chronic infectious disease, affects to varying degrees all animals. The figures given in this work are for tuberculosis in general, not only of the lungs, but of any tissue to any extent of lesion; thus only can we measure comparative susceptibility. Man and the rest of the Primates, except the marmosets, are especially prone to it, and the apes and monkeys exhibit practically no resistanc to it. From time immemorial the tall, thin, flat chested, narrow bodied man has been the type in which tuberculosis of the lungs is expected, features that are confirmed in part by modern anthropometrics. The greatest number of cases in our series is to be found among the primates, and of these the Old World monkeys and the apes have the greatest incidence. The next positions are held by the ungulates, bovines and deer, with the viverrine cats a poor fifth. Analysis of these distributions shows at once that mesosomic, mesosplanchnic animals have the first place, macrosomic, megalosplanchnic animals the second. The trunk type of the Simiidæ is the only one of these groups that approaches that of man; its conformation, however, especially its width to anteroposterior depth relationship, is definitely other than human in being greater in depth. The other three families of Primates have a laterally contracted chest, ovoid in cross section. The heart-to-lung relationship is much the same in all families with the noteworthy exception of the Simiidæ in which the heart is more firmly attached to mediastinum, lungs and diaphragm, and the bronchial tubes are shorter, narrower and sturdier. The remaining groups have rather "loose" hearts, wide bronchi and probably greater richness of vascular bed.

It will be noticed that among the wild animals the herbivorous varieties occupy the most prominent places and those of the ungulates with a compound stomach have a greater incidence than those with a simple organ. Lastly one might

mention that the susceptible varieties have a trunk, *i.e.*, a fulcrum, large for the extremities. In man, the neck of the tuberculosis constitution is relatively long. In the lower animals this finds agreement (Ungulata) and disagreement (Cercopithecidæ).

Ductless Glands.—Alterations of these important structures are subjects of physical as well as pathological study since they are constantly occurring under nutritional, seasonal, procreative and adjustmental conditions. In no other panel, or anatomical, physiological or psychological subdivision of the animal body does the question of qualitative and quantitative balance loom so large. The exact data about our cases are anatomic only so that after mentioning the important features of our tabulation, I shall analyze our information somewhat arbitrarily according to the groups accepted by Pende as representing types dominated by one or another unit of the ductless system.

The Carnivora lead by percentage all other mammalian groups in showing pathological changes in these organs. The thyroid body, so important in human goiter, leads the list. Domestic dogs are well known to suffer from goiter and the wild dogs are equally or more definitely susceptible, at least in captivity. How far inbreeding in our dogs and raccoons may affect these figures is a matter for speculation. The opossums are the only other mammals to give a high value for thyroid pathology. It seems worthy of special mention that the rodents are almost immune to morbid endocrine disturbance and that the ungulates have little of it. The primates occupy a middle position. Those animals that have the most endocrine disturbance have relatively high skeletal disease and tumor figures, and low tuberculosis. Endocrine disease and tuberculosis do not go hand in hand.

The human hyperthyroid type is dolichomorphic and microsplanchnic with a disproportionately large heart, and of rapid bodily action. This description would hardly fit the dogs, and not at all the raccoons that are second in line. The lower zoölogical type that suffers with morbid changes in the

thyroid is mesosomic with a disproportionately developed head, skeleton and musculature (carnivores and higher marsupials). The primates that have goiter are dolichomorphic and microsplanchnic as in man.

The Canidæ correspond also in part to the human hypothyroid habitus in being heavy set with well developed skeleton and tendency to adiposity. Some definite examples of cretinism have been seen in this family.

The human hyperpituitary type is macrosomic but docholimorphic with a heavy skeleton and thick tissues; it has a tendency to cardiovascular disease. The families having pituitary disease in our list, the bovines, deer, antelopes and camels, are indeed not far from the human type, comparatively speaking, but caution is certainly needed here because not every pituitary has been examined and we have had but six cases in these groups.

We have no dependable data concerning the hypopituitary type, nor can we make fair statements concerning deficiencies or excesses of function in adrenal, parathyroid or thymus.

The Skeleton.—As with the foregoing organs caution is to be observed in allotting to any group a susceptibility to bone disease, since the skeleton is a tissue undergoing change in the lower mammals longer than in man, at least for a longer percentage of life, and is constantly under the influence of the endocrine glands and of the diet. We have demonstrated beyond peradventure that bone disease in the monkeys proper can be very largely controlled by adjusting diet to them, especially in offering balance of inorganic matter and easily used protein. Our figures indicate that primates, ópossums raccoons and squirrels are most vulnerable as to their bones, but this may be misleading because several of the cases have been in young born in the Garden and with a mother perhaps inadequately fed. Nevertheless, it is permissible to quote them as having "susceptible" bones. There is no special zoological type that stands out of this number, but it should be noted that they are meso- and microsomic animals with disproportionately small skeleton and delicate periosteum.

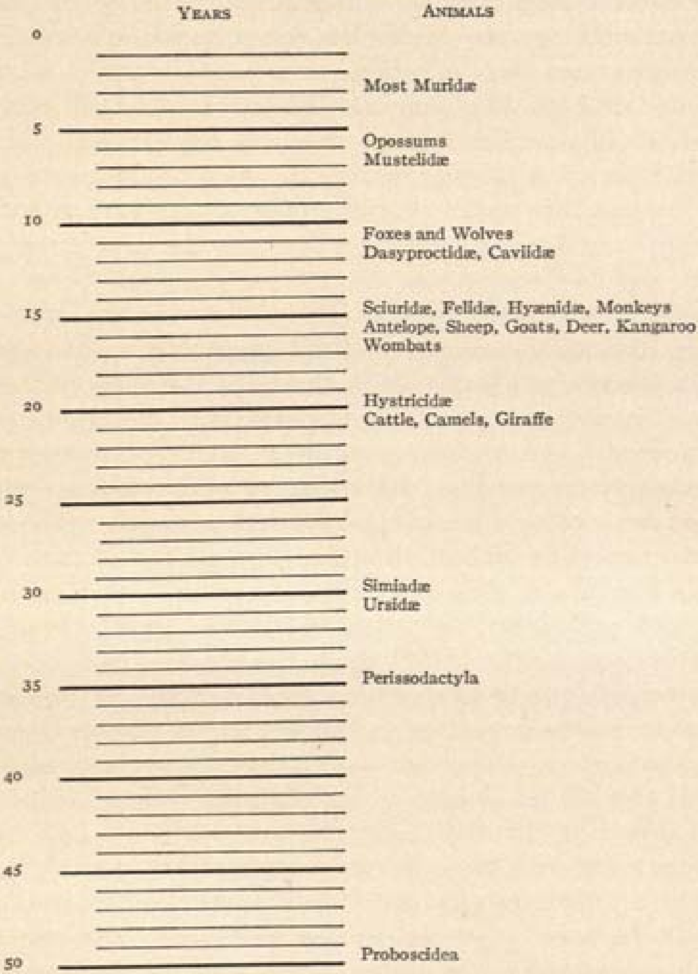
Tumors.—These genetically regressive but physically progressive changes have no specific anthropometric status nor outstanding zoölogical anatomic distribution. The greatest numbers are to be found, in order, in the Carnivora, Rodentia and Marsupialia, and in these orders in the carnivorous families. The bovines take the fourth place. Thus we find meso- and microsomic varieties predominate, but without unequivocal association with the anatomy or pathology.

Analysis of Separate Units of Constitution

Age.—Since the exact age of our specimens is known in so few instances, an analysis with this as a standard cannot be made. It may be replaced by investigating the morbid lesions of the orders and families in terms of expected or potential longevity, for which may be taken, as we have done before, the figures as worked out by Chalmers Mitchell. The ages of the animals under consideration show great variation even within families, so that an absolute separation is not feasible. A rough guide may, however, be found in the scheme on opposite page.

This "order of potential longevity" permits less than was hoped for, largely because of the variability within families. The animals with the shortest life expectancy have no very remarkable vulnerability in the subjects under study, but such as it is, it affects the kidney, intestinal tract and tumor growth. The next two, that are close together, opossums and skunks, otters and badgers, also present no outstanding peculiarity unless it be in the ductless glands and skeleton. Much the same can be stated concerning the varieties that are next in order, except that there is most marked among them the gastrointestinal dominant. The varieties that show the greatest ductless gland vulnerability fall into the lower half of the life expectancy list, a fact that recalls what was formerly noted that endocrine balance and imbalance have an effect upon longevity. With the fifteen-year expectancy animals, arrives a somewhat greater tendency for chronic

SHOWING APPROXIMATELY POTENTIAL LONGEVITY IN ORDER AND IN RELATION TO YEARS



disease as indicated by definitely more tuberculosis, skeletal degenerations and tumors. What does not appear in the tables is that chronic vascular and renal diseases are definitely greater, beginning at this time. This type of reaction continues in the twenty and thirty year groups with the addition of a gastrointestinal feature among those varieties that live the longest—apes, bears and odd-toed ungulates. To what

extent longer life contributes toward the development of these chronic conditions by allowing time for them to occur, and how much is to be ascribed to pure organic or physiologic constitution, are open questions.

Carnivores vs. Herbivores.—The relationship of dietary habits, exclusive of zoölogical order, to organic and physiological disease is almost unequivocal. The carnivores present a greater incidence of disease in all four visceral systems here discussed and have a greater tendency to ductless gland disorders and to tumors. In diseases of the skeleton and in tuberculosis the herbivores lead. By mathematical analysis of the individual figures, the only exception to the above general statements is that while there are more cases of heart disease among the meat eaters, lesions of this class were contributed in greater degree by the herbivores, a fact that is due entirely to the high susceptibility of the bovine heart. The association and significance of this fact have been commented upon in another section.

Uniparity vs. Pluriparity.—The results of analyzing the incidence of our test diseases in this respect is about as definite as in the foregoing instance. The animals that bear a single young as a normal character are the Primates, the seals and the ungulates except pigs and peccaries. All the rest give birth to more than a single offspring. The pluripotential animals have more heart, kidney, respiratory, endocrine and neoplastic diseases while tuberculosis and bony changes are more numerous in the unipotential.

There follow in the succeeding paragraphs analyses of several features that cannot be put into mathematical formulæ with the definiteness with which the preceding statements have been made. Nor will it be possible to group the animals on a zoölogical and at the same structural basis as heretofore. The statements are therefore more in the nature of generalizations which it is hoped will lead to further inquiry and confirmation or correction.

Body Size.—A division into groups according to size is rendered invalid because of greatly differing body bulks

arrived at within the same family and genus. Taking those that reach the greatest size, bears, seals, lions and ungulates, it is noted that they suffer most with diseases of the kidney and intestinal tract. Small mammals, as monkeys, opossums and the smaller carnivores, show vulnerability in the respiratory tract, skeleton, ductless glands and neoplastic diseases. It is noteworthy that tuberculosis is found mostly in small and in the largest mammals. Examination of the figures for mammals with long body and thin legs, such as deer, antelope and monkey, shows a vulnerability in the respiratory and intestinal tracts and for tuberculosis. Small, short-legged, stocky animals, as the beavers, porcupines, devils, raccoons, have a tendency to renal disease, endocrine alterations and tumors, but do not have in general great susceptibility for the morbid processes under discussion. Light, thin, quick species, as mice, rats, South American monkeys, cavies and squirrels, have much renal and skeletal disease.

Hibernation.—No very remarkable pathological feature characterizes this group. There are only five of the families studied here in which hibernation is a family character. These families are Ursidæ, Mustelidæ, Viverridæ, Sciuridæ and Chinchillidæ. Attention is directed only to the fact that of the 311 animals with this habit, 4 per cent show ductless gland diseases and tumors and 5 per cent show bone diseases.

There are only three essential differences in the distribution of the disease conditions in these studies between animals having claws, paws, hoofs or hands; these concern tuberculosis, the ductless glands and tumors. Tuberculosis affects chiefly those with hands and hoofs, while claw-bearing animals suffer with more endocrine disturbances and tumors.

Attempt was made to show morbidity relationships of the facial angle, and the position of the ears to the scalp. It was found that these figures correspond almost exactly with those already discovered in study of the ordinate and familial disease indices. So, too, the investigation of those animals with well developed canine teeth versus those devoid of the excessively pointed tearers, revealed that ordinate characters were

5

maintained. However, the figures for the two great groups with the best, that is, longest and most deeply anchored canines, the Cercopithecidæ and the Carnivora show an unexpected finding. In practically all cases, more diseases of our main subjects, except tuberculosis, were found in the Carnivora, but the herbivorous, Old World monkeys contributed a greater percentage of diagnoses to the total for each subject studied, except in the case of renal disease and the ductless glands.

Thorax.—There is no essential pathological tendency characteristic of the barrel-shaped, blunt wedge-shaped and keel-like thoraces. To contrast the least similar, that of the ape and monkey with that of the ungulate, we find the heart diseases of the latter greatly in excess, while tuberculosis is more prominent in the former.

Abdomen.—This may be described as straight, as in the monkey, pendulous, as in the ape and horse, retracted, as in the dog. There is no association between this and the diseases under discussion.

In the foregoing pages an attempt has been made to illustrate some of the possibilities of the study of animal constitution as reflected by certain arbitrarily adopted morbid changes observed during the past twenty-five years at the Philadelphia Zoölogical Garden Laboratory. Partly this study has been made by observation and experience, but in much larger part by analysis of tabulations made from our extensive filing system.

And how shall these studies be used? Nothing is more alluring than conclusions, nothing more seducing than deductions, nothing more dangerous than generalizations, for they are apt to lead to great errata. Much better for such work to be used as collected and related beginnings from which emanate more questions, more phases, more understanding and fewer conclusions.

The purpose of a relief survey of this kind, exposing as it does an unusual method of study, can in itself be a conclusion. By such means, a rough guide of qualitative individualism

within the meaning of our definition, may be gained to indicate the ability of animals to react to the agencies that influence their bodies and lives.

It might be asked, as indeed I have been asked, whether or not this bears on evolution. That certainly remains for others to settle. It is no new thought that pathology is the basic reason for evolutional beginnings and variations of both large and restricted groups; but that such immediate organic responses as are described in this work can have a real influence in zoölogical alterations may perhaps be answered by students of emergence. However, I venture to ask if such material is not a better guide to the significance of pathology in evolution than is the pathology of man? Man today is heterogeneous, or at least less homogeneous than are most lower groups. Is this position very different from that of the man who has not been popular for many years, but whose work is still used and quoted by all biologists, that great biological pathologist or pathological biologist, as you will, the centenary of whose death we observe this year—Lamarck? While the subject is left as raw material, it is a profound hope that something has been put on record that will lead to scientific speculation followed by practical application, and that has been worthy of this distinguished society in these impressive surroundings.

LINDENIOPIPER, A GENERIC SEGREGATE FROM PIPER

By WILLIAM TRELEASE

(Read April 19, 1929)

RESTRICTION of the tribe Pipereæ to all polycarpellary 1-ovulate Piperaceæ which do not produce an axillary inflorescence, still brings together three distinct groups of species that are referred commonly to *Piper*. Though very heterogeneous, and comprising several subgenera that have been segregated generically at one time or another, the constituents of this genus, as so defined, have spikes that are sympodially opposite the leaves.

It is probable that the name *Piper* will be restricted finally to the Old World species with imperfect flowers subtended by commonly round-peltate bracts. Meantime it seems difficult to draw a demarcation line between these and the other sessile-flowered groups with similar inflorescence; but a series of American species with flowers pedicellate in the spikes may be kept apart readily on this character, under the generic name *Ottonia* which Sprengel applied in 1820.

The purpose of the present short communication is to segregate from the sessile- and perfect-flowered Pipers a third genus of Pipereæ, for which the name *Lindeniopiper* is proposed in memory of the discoverer of its only known representative which was named *Enckea Lindenii* by Miquel in 1844, and *Piper Lindenii* by Casimir de Candolle in 1869. Unlike all other Pipereæ, *Lindeniopiper* bears one or two spikes *on a 1-bracted peduncle* opposite each fertile leaf, the peduncles of all others being bractless, so that the inflorescence here is obviously or constructively compound. So far as known, its single representative, a small tree, *Lindeniopiper Lindenii* (Miq.), occurs locally about Teapa in the southern

Mexican State of Tabasco. In aspect it resembles a small group of gibbous-leaved Enckeas in the genus *Piper* as now accepted, and it has been figured in a general way in Miquel's Illustrationes Piperacearum, pl. 66, published in 1846.

URBANA, ILLINOIS
March 11, 1929.

GRAVITATION

By CHARLES F. BRUSH

(*Read April 19, 1929*)

At the Minneapolis meeting of the American Association for the Advancement of Science in December, 1910, I had the honor to outline "A Kinetic Theory of Gravitation."[1] This was followed by a "Discussion" of the theory in 1914.[2] A second "Discussion" came in 1921.[3] A third "Discussion" appeared in 1926.[4]

The latter paper contains a concise synopsis of the theory and very convincing argument supporting my contention of 1910, that the energy acquired by falling bodies is derived from the ether.

This Kinetic Theory of Gravitation postulates that the ether is endowed with vast intrinsic energy in the form of waves propagated continually in every conceivable direction, so that the wave energy is isotropic. (The belief is expressed that *all* energy is primarily energy of the ether.)

The very high-frequency ether waves, which embody most of the ether's intrinsic energy, pass freely through matter without obstruction except that concerned in gravitation, and a very small heating effect (which will be explained later). The ether waves exert motive action on atoms or particles of matter whereby the latter are buffeted about in all directions with some absorption of etherial energy. Thus a lump of matter casts a spherical energy shadow into space, the depth of shadow diminishing with the square of distance from its origin. The energy shadows of two or more bodies inter-

[1] *Science*, March 10, 1911; *Nature*, March 23, 1911.
[2] *Proc. Am. Phil. Soc.*, Vol. LIII, No. 213, January–May 1914.
[3] *Proc. Am. Phil. Soc.*, Vol. LX, No. 2, 1921.
[4] *Proc. Am. Phil. Soc.*, Vol. LXV, No. 3, 1926.

blend, so that energy density between them is less than elsewhere, and they are *pushed* toward each other by the superior wave energy from directions beyond them. The 1926 paper [4] explains this at length.

To aid in forming a mental picture of the relation of the very high-frequency ether waves postulated as the cause of gravitation, to other well-known classes of ether waves, I have prepared the chart of ether-wave frequencies shown in Fig. 1.

Each horizontal line in the scale of frequencies represents double the frequency of the line below it, or half the frequency of the line above it. Thus the scale of frequency increases upward by octaves as in music.

Starting at the bottom of the scale with a frequency of one ether wave per second, the first line above represents two waves per second, the second line four waves per second, the third line eight waves per second, and so on to the tenth doubling where we get a frequency of 1,024 waves per second as shown. Continuing the doubling process another ten times we get a frequency of 1,024 times 1,024, or 1,024 to the second power, and so on upward in the scale to the third, fourth and fifth, etc., powers of 1,024. Thus it is seen that the indicated wave frequency increases with great rapidity as we ascend the scale. At the twentieth octave it is more than a million per second; at the fortieth octave more than a million million waves per second.

I am indebted to a chart shown at the British Exposition in 1925; to a chart by G. L. Clark in 1927, and to a chart by W. E. Deming in 1929 for much of the material shown in my chart. But I have arranged it somewhat differently, emphasizing ether-wave frequency rather than wave-length.

Frequency is converted into wave-length by dividing the velocity of light per second by frequency of the waves per second. Thus the frequency 1 at the bottom of the chart means one wave per second, and its wave-length is the distance it would travel in one second, before another wave started after it. This is the velocity of light, about 186,000

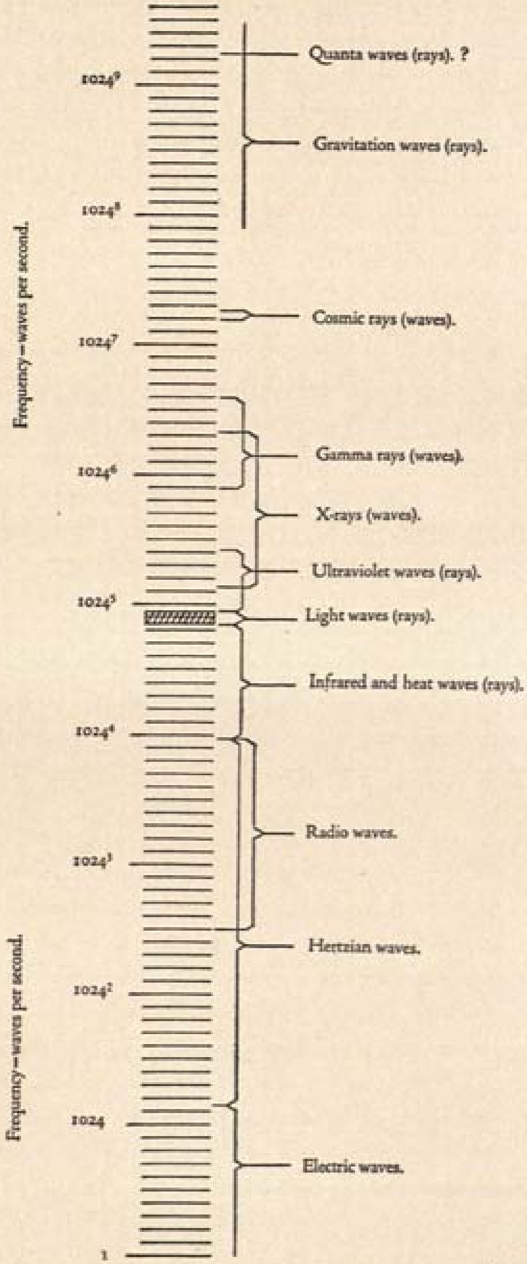

Quanta waves (rays). ?
Gravitation waves (rays).
Cosmic rays (waves).
Gamma rays (waves).
X-rays (waves).
Ultraviolet waves (rays).
Light waves (rays).
Infrared and heat waves (rays).
Radio waves.
Hertzian waves.
Electric waves.
Frequency—waves per second.
Frequency—waves per second.
1024⁹
1024⁸
1024⁷
1024⁶
1024⁵
1024⁴
1024³
1024²
1024
1

FIG. 1

miles, or about 300,000 kilometers. (All ether waves travel with the same velocity.)

A wave-train of this very low frequency and very great wave-length could easily be launched into the ether by revolving a closed coil of wire on its own diameter as an axis, in a magnetic field, at the rate of one revolution per second. If we should increase its revolutions to ten per second, we would get a frequency of ten, and a wave-length of 18,600 miles. Such mechanically generated electric ether waves may be increased in frequency without much difficulty as far up as shown in the chart.

Next above we have Hertzian waves, covering about 28 octaves, generated by the condenser and spark-gap method. The upper half of this long range of ether waves contains the waves used in radio transmission of speech and music.

Next above the Hertzian waves, of higher and higher frequency and shorter and shorter wave-length, we find the infrared and heat waves covering about 9 octaves. These waves embody most of the heat received from the sun, and nearly all the heat radiated from hot bodies below redness.

Then we come to the exceedingly interesting and intensively studied light waves or rays. These cover barely one octave of the scale, and their mean frequency is about five hundred million million waves per second. It seems unfortunate that the human eye is sensitive to such a short range of ether wave vibrations only, while the human ear can perceive about eleven octaves of sound, or air vibrations. Perhaps some animals or insects have a wider range of vision than humans.

Above the light waves we find about five octaves of ultraviolet waves. The sun's radiation includes the last three classes of waves, though some of the ultra-violet is absorbed by our atmosphere and does not reach the earth. The ultraviolet rays or waves promote chemical action and are chiefly responsible for the ordinary photographic image.

Next in the growing range of frequency we have the well-known X-rays, so extensively used in X-ray photography for

therapeutic and industrial purposes. These cover a long range of octaves in our chart, and overlap the upper part of the ultra-violet range and the lower part of the gamma range. X-rays, particularly those of the highest frequencies, pass rather freely through large thicknesses of light substances such as wood, fabric, animal tissue and metals of low atomic weight and density; but metals of large density and high atomic weight absorb and obstruct them greatly. Thus a quarter inch of lead almost completely stops X-rays of the highest frequency. The ability of X-rays to pass deeply into or through matter, is called "penetrating power."

Above the X-rays in our chart are the gamma rays of radium, so extensively used in therapy. These have a much greater penetrating power than the highest-frequency X-rays.

All the above described classes of ether waves have been demonstrated experimentally, and doubtless all exist to some extent, permanently in the ether of space. Particularly is this true of the heat waves, as I pointed out in my 1927 paper.[1] In that paper I showed, conclusively I think, that a lump of matter far out in inter-stellar space could not possibly fall to absolute zero of temperature by radiation of all its heat, as commonly supposed, but would soon acquire and then maintain the "temperature of space" which I estimated to be something like 50° to 100° above absolute zero.

Above the gamma rays there are about six octaves which have not yet been explored experimentally. Then we come to the cosmic rays, so ably demonstrated and studied by Dr. Millikan. These cover considerably less than one octave, and their mean frequency is about five thousand million million million waves per second. This is ten million times greater frequency than light waves possess; and yet there can be no doubt that cosmic rays are ether waves like all the rest. As might be expected, cosmic waves, on account of their very much higher frequency (shorter wave-length), have far greater penetrating power than the highest-frequency X-rays; in fact, about 300 times greater, as they pass through six feet of lead.

[1] *Proc. Am. Phil. Soc.*, Vol. LXVI, 1927.

Starting considerably above cosmic rays in the chart, I have drawn a long bracket with indeterminate ends. Somewhere in this region lie the isotropic ether waves of gravitation, probably having considerable range of frequency. The enormous frequency of these waves enables them to pass freely through all kinds of matter without obstruction except that concerned in gravitation.

Probably most of the vast intrinsic wave energy of the ether lies in the region of the gravitation waves.

Until about a year and a half ago, we had no experimental evidence of the gravitation waves other than gravitation itself. But gravitation is a most impressive demonstration of the ether waves which cause it, and of the very great energy embodied in them. As illustrating both points, I call attention to Lord Kelvin's graphic word-picture of collision of two large astronomical bodies under the influence of their mutual gravitational attraction, which I have quoted in my 1914 paper,[2] and my 1926 paper.[4] As another illustration of the enormous differential ether-wave *push* of astronomical bodies toward each other, let us consider the case of the earth and moon. The urge toward each other is commonly called gravitational attraction, which is only another way of looking at it. If this attraction were absent, and the moon were held in her orbit by a weightless steel cable, the cable would need to be about 500 miles in diameter to stand the strain. Between the earth and sun, the cable must be about 6,000 miles in diameter. And the attraction (push toward each other) of the components of some double stars must be thousands of times greater than this.

Obviously, the ether waves of gravitation, and the other classes of waves we have discussed, must be permanent attributes of the ether; they cannot escape either from boundless or bounded space. They must fill all space; and we may therefore regard gravitation as a property of space, because wherever there are two or more particles or bodies of matter, however small or large, however near or distant, they are urged toward each other by the ever-present isotropic ether waves of gravitation.

Very high up in the chart I have tentatively drawn the line marked "Quanta waves or rays," indicating a frequency of 6.554×10^{27}.

Experimental Evidence of the Ether Waves of Gravitation

A year ago I had the honor of presenting a paper under the title "Correlation of Continual Generation of Heat in Some Substances, and Impairment of Their Gravitational Acceleration."[1]

This division of the present paper is a continuation of last year's paper; and to save the reader the bother of looking up that paper and its several references, I shall quote very freely from it and prior papers.

The third "Discussion"[4] (1926) contains in its title "Some Experimental Evidence Supporting Theory; Continual Generation of Heat in Some Igneous Rocks and Minerals. Relation of this to the Internal Heat of the Earth and Presumably of the Sun." Quoting from this paper, "Gravitation Waves and Heat":

"Heat is often defined as an agitation of atoms and molecules of matter, and measured by the total kinetic energy of such agitation. The agitation consists partly in internal vibrations of the elastic atoms and molecules and spinning about their various axes, and partly in a very rapid translatory motion among themselves. Thus they are supposed to dart about in every conceivable direction, constantly colliding with each other and rebounding or glancing in new directions. The kinetic energy of this translatory motion constitutes *sensible* heat (not total heat) and is the measure of *temperature*. Anything (such as absorbed radiation) which stimulates the internal vibration of atoms or molecules likewise increases their translatory velocities by the increased violence of rebound after collision, and thus increases their temperature; and *vice versa*.

"All the above is known to be true of gases and vapors (Kinetic theory of gases), and is generally believed to be true of liquids and solids.

"The '*mean* free path' and the '*mean* velocity' between collisions of the molecules of many gases under stated conditions have been computed. But it has also been shown mathematically that the higher and lower velocities, and the longer and shorter paths,

[1] *Proc. Am. Phil. Soc.*, Vol. LXVII, No. 2, 1928.

differ greatly from the means, and may in each respect vary twenty or more times in amount. Doubtless this is true also of liquids and solids.

"From the fortuitously wide variation in velocities and free paths of the billions of vibrating atoms or molecules in their heterogeneous movement, it follows that collision frequencies must also vary greatly, from instant to instant, everywhere in a body of matter.

"Probably the postulated gravitation waves are not confined to one frequency, but have a wide range of frequencies as do the well-known X-rays.

"With the foregoing in mind it is easily conceivable that some kinds of matter may have atoms or simple molecules or complex molecules of occasional vibration frequency corresponding with some gravitation wave frequency, whereby fortuitous resonance can, for brief instants, be established at various points. This would result in a slight increase of vibrational activity and a cumulative rise of general temperature.

"A body of such matter, with some thermal insulation, would become and remain permanently warmer than a neighboring body similarly circumstanced, but not endowed, or less endowed with the permissive heat-generating quality."

A carefully designed calorimeter is illustrated and described in the paper (1926), and details of many experiments given. These resulted in the discovery that some rocks and minerals did generate an easily observable amount of heat.

In April, 1927 I presented another paper on "Persistent Generation of Heat in Some Rocks and Minerals." [1] This is a continuation of the 1926 paper. It describes a new and different calorimeter, built in the spring of 1926, and since known as the "Ice Calorimeter." It has been in almost continuous use down to the present time (April, 1928) and has proved very satisfactory. With this calorimeter it has been found that some of the natural heat-generating materials, and some of the artificial silicates hereafter described, have retained their heat-generating activity unimpaired; and none of these substances is more than minutely radioactive. Quoting from the 1927 paper:

"It is notable that all the materials which appear to be endowed with persistent heat generating activity are complex silicates."

There follows a description of the preparation, in the wet way, of many complex silicates, and their preliminary testing for heat generation. A silicate of the protoxides of nickel and cobalt showed

[1] *Proc. Am. Phil. Soc.*, Vol. LXVI, 1927.

very large activity, larger than either silicate alone; and this now appears to be permanent. Nickel and cobalt are almost identical in atomic weight, and differ but one unit in atomic number. Quoting again from the 1927 paper:

"In the absence, at present, of other explanation, it is thought that persistent heat generation in some rocks and minerals is due to isotropic ether waves of great penetration; very great indeed, if the generation goes on in the interior of the sun and planets as it does at the surface of the earth." Quoting now from the 1928 paper:[6]

"It is now believed that the class of isotropic ether waves postulated as the cause of persistent generation of heat in some substances, is the same class, perhaps of very wide range of frequency, postulated as the cause of gravitation.

"Conversion into heat of some of the energy of the gravitation ether-waves, however little, might be expected to impair to some extent the falling velocity of a heat generating substance; *and all such substances thus far tested have clearly shown impairment.*

"I have yet found no exception to this remarkable phenomenon, though I have already tested many natural and artificial minerals. Substances which have shown no generation of heat in the calorimeters show no impairment of their falling velocity when compared with lead. Substances exhibiting small, moderate or large generation of heat have shown comparatively small, moderate or large impairment of their gravitational acceleration.

"In making the above indicated comparisons of falling velocities I have largely used the method and apparatus described and illustrated in my 1923 paper on 'Some New Experiments in Gravitation.'"[1] (See also 1924 paper of same title.)[2]

"Two aluminum containers are used, alike in size, shape, weight and smoothness of surface, and dropped *simultaneously*, side by side, through exactly the same distance (about 122 cm.).

"Each container, at the end of its journey, breaks an electric circuit. But the breaks of both containers are in series in the same circuit, so that the break which occurs first produces a bright spark, while the belated break gives no spark because its circuit is already open.

"When the containers are equally loaded with the same metal, there is no visible spark at either break, or a very feeble spark at one or the other indifferently. But when they are equally loaded with certain different metals, one container persistently produces a

[1] *Proc. Am. Phil. Soc.*, Vol. LXII, No. 3, 1923.
[2] *Proc. Am. Phil. Soc.*, Vol. LXIII, No. 1, 1924.

bright spark, though the containers are always reversed in position for each trial. From this it seems clear that the container giving the spark falls a little faster than the other. This sparking condition is clearly manifested when the faster container reaches the end of its free path as little as .0125 mm. (.0005 inch) in advance of its neighbor.

"The 1923 paper also describes how approximate quantitative measurements are made. These are very tedious, especially when falling velocity differences are large.

"To facilitate estimation of the larger falling velocity differences I am perfecting a photographic method of observation. After falling about 110 cm. the small lower ends of the containers are photographed in silhouette against a white background having many horizontal black lines, and illuminated by a very bright electric spark."

Then follows several pages of text, with figure and plates, describing the apparatus and its operation, too lengthy to quote here. Resuming quotation from last year's paper (1928):

"The camera lens is located about 37 cm. in front of the white surface, and the photographic plate about an equal distance behind the lens; so that the image is about equal in size to parts photographed.

"The plate holder moves vertically in guides, and rests on a pin in one of eight equally spaced holes 1.6 cm. apart in the back-board of the camera. This back-board has a horizontal opening 1.4 cm. wide, which limits the exposed portion of the plate to a strip of this width. Thus eight pictures of the falling container tips are made on one plate. The containers are reversed in right and left position after each exposure.

"Plate III shows such a series of photographs. Each container weighed approximately 30.6 grams. One was marked with a white spot on its top for identification. This one, lettered S on the plate, was filled with silicate of nickel and cobalt, which weighed 13.6 grams, or about 30.8 per cent of the total weight of the loaded container. The unmarked container was loaded with lead sawdust, held tightly in the lower end by a short closely fitting cork above it, until it very closely equaled the marked container in weight.

"Each of the eight photographs on the plate, when magnified, clearly shows the S container (Silicate) slower than its companion. Six more similar plates have been made with the same loaded containers, and all show the same effect. It will be noticed that the amount of retardation of the S container varies considerably in the eight exposures of Plate III. This was principally due to small

lateral air currents in the room which acted unequally on the two containers when one shielded the other; as was demonstrated with another plate by purposely increasing the lateral air currents. I shall eliminate lateral air currents in future work.

"Of course I tried exchange of loads in the containers, but without observably affecting the result; the container holding the silicate was always slow.

"The observed retardation of the silicate container must be due to impaired gravitational acceleration of the silicate as compared with the lead sawdust in the other container; and as the silicate constitutes only 30.8 per cent of the total mass undergoing acceleration, we must multiply the observed retardation by 3.25 to find the full impairment of the silicate alone.

"In the apparatus as set up, the centers of the container tips are about 1.6 cm. in front of the lined background; hence tips and lines cannot both be sharply in focus of the camera lens. In Plate III the focal plane of the camera was about half way between the tips and the lines. Sharpness of lowest part of the curve of the tips was very greatly enhanced by permanently covering all of the camera lens except a horizontal strip 2 mm. wide across its center.

"The comparison lines in Plate III are spaced one mm. apart between centers. I am installing another white background with very much finer lines spaced only half a mm. apart, and far better adapted to micrometer measurement of container tip differences of level.

"The Bureau of Standards, with a calorimeter of its own designing, is working with some of the heat-generating substances for the purpose of checking my findings."

Since writing the 1928 paper I have continued work on the fascinating subject of "Correlation of continual generation of heat in some substances and impairment of their gravitational acceleration." These phenomena appear to be related as cause and effect. I have gathered considerable new evidence, all affirmative, so that I now feel justified in dismissing doubt.

In the quest for more evidence, however, I have reversed the order of procedure; *i.e.*, instead of hunting more or less at random, for new substances exhibiting generation of heat in the calorimeters, which is a very slow and tedious process, I have first tested many new substances for impaired gravitational acceleration, which is comparatively rapid and easy.

In these tests I have made more than fifty new plates, and feel well rewarded for the labor.

In my former experiments it had appeared that silicates of the iron group of metals exhibited much greater impairment of acceleration than silicates and compounds of the metals of lower atomic weight. So I chose silicates and other compounds of metals of still higher atomic weight, viz., Barium, Lead and Bismuth as most promising materials to work with.

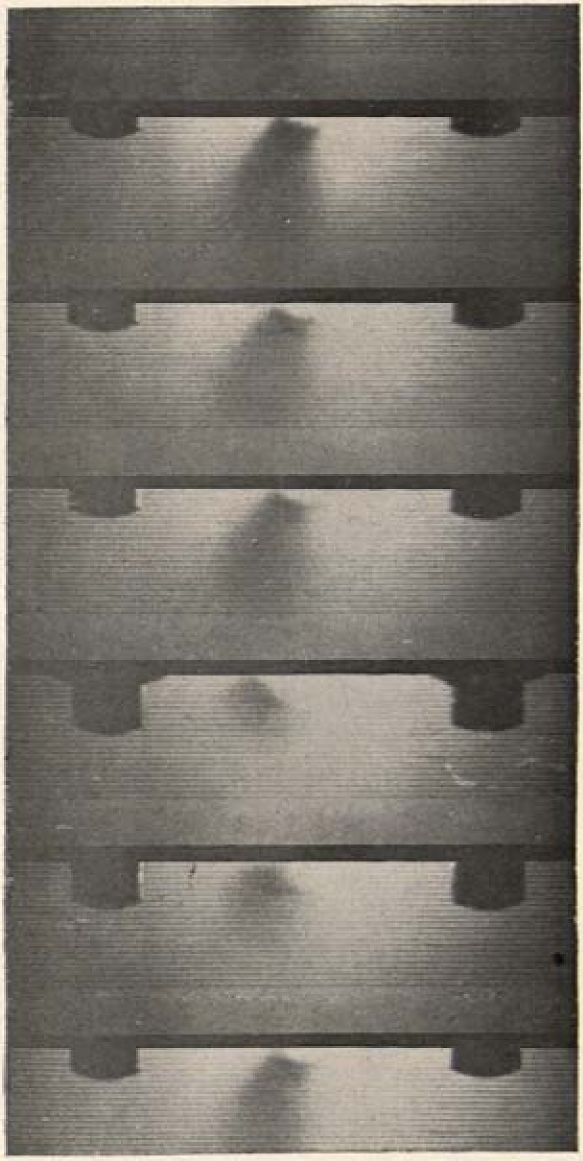
In all the artificial silicates there was some sodium silicate. Lead silicate gave a moderate effect, but after ignition none. Bismuth silicate behaved in the same way. Lead acetate gave a moderate effect.

The Barium compounds were found very interesting, and were more fully investigated. Barium Hydroxide, $Ba(OH)_2$, $8H_2O$ gave rather large effect, but required lining of the container on account of its powerful corrosive action on metallic aluminum. BaO (anhydrous); effect moderate. BaO, $8H_2O$; effect small but certain. $BaCl_2$, $2H_2O$; effect small. $BaBr_2$, $2H_2O$; effect very small if any. $BaSO_4$; effect very moderate. Ba_3, $(PO_4)_2$; effect small. $Ba(SCN)_2$, $2H_2O$; effect very small if any.

The Barium Aluminates were the most interesting of all the compounds examined. Three grades were prepared: $(BaO)_2Al_2O_3$, BaO, Al_2O_3 and $BaO(Al_2O_3)_2$. All, after air drying to constant weight, lost several per cent of hydroscopic moisture when dried at 100° C. and several more per cent of combined water when gently ignited. After ignition they were but very slightly hygroscopic. The BaO, Al_2O_3, dried at 100° C., gave largest effect; but after ignition the effect was somewhat reduced.

Plate I shows the impairment of gravitational acceleration in the BaO, Al_2O_3, not ignited, as compared with lead in the usual way. The upper two of the usual eight photographic strips are omitted, in order to permit enlargement of the remaining six strips about fifty per cent. The black lines on the white background against which the lower tips of the two falling containers are photographed, are spaced exactly half

PLATE I



S

S

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a millimeter apart between centers, and are as fine as it was found practicable to make them. As before explained, neither the lines nor the tips of the rapidly falling containers are sharply in focus of the camera lens. The tip of the container holding the Barium Aluminate is marked S (slow) on the plate; and the containers were reversed in position after each dropping as indicated. As easily seen on the plate, the S container is unevenly slow in the six photographs, and even very slightly fast in the first. This unevenness is attributable to variation in the exceedingly slight friction of the containers in their guiding tubes during the first millimeter of their fall. The containers are perfectly free after that.

For accurate measurement of falling velocity differences of the containers on all plates, I have used a binocular microscope of low magnifying power, having a very large stage provided with a high-precision micrometer specially designed and built for this purpose.

In finding the average impairment of gravitational acceleration of the S container in the six photographs of Plate 1, all were measured with the micrometer, the five affirmative values were added together, the slight negative value of the first one subtracted from the sum, and the remainder divided by six. This gave the mean slowness of the S container = .099 mm., say one tenth of a millimeter. This is one part in eleven thousand of the distance fallen (110 cm.). But the Barium Aluminate constituted only 40.3 per cent of the total weight of the loaded container. Hence, impairment of falling velocity of the Barium Aluminate alone, as compared with equal weight of lead in the other container, was one part in about 4450. When a companion plate was made with the container loads exchanged (as customary in all tests), slowness of the Barium Aluminate was found closely the same.

Of the several compounds described and tested for impairment of falling velocity, only two have been tested in the ice calorimeter for continual generation of heat. Barium Sulphate, the first of these, was chosen because of its undoubted

stability, although it had shown but very moderate impairment of falling velocity. It exhibited very moderate but steady and satisfactory generation of heat during a long run in the calorimeter. The second calorimeter test was made with Barium Aluminate like that used for Plate I; but after preparation it was only air dried at room temperature, to avoid any instability that might arise from hot-air drying, or ignition. The specimen was prepared about two months ago, and has been in the calorimeter during the last six weeks, where it continues to show rather large and steady generation of heat. It appears to be quite stable.

More than a year ago the Bureau of Standards very kindly offered to repeat some of my experiments on "continual generation of heat in some substances." A special ice calorimeter, quite different from mine, was designed and built for the purpose. After much time spent in perfecting and calibrating the calorimeter, a specimen of the air-dried Sandusky clay described in some of my earlier papers, was tested during the last few months; and I have very recently received official announcement from the Bureau in confirmation of my finding that this substance does continually generate a measurable amount of heat. The Bureau is about to commence testing the comparatively very active Nickel-Cobalt Silicate described in my last two papers.

Correlation of continual generation of heat in some substances and impairment of their gravitational acceleration, is regarded as very strong evidence in support of the kinetic theory of gravitation; and we seem now well on the way of finding out something definite about the nature of gravitation, which has been by far the greatest of all outstanding physical problems.

CLEVELAND, April, 1929.

AN EXPERIMENTAL INVESTIGATION OF THE PHYSICAL NATURE OF DEATH

By GEORGE W. CRILE, MARIA TELKES AND AMY F. ROWLAND

(*Read April 19, 1929*)

THE terms 'living' and 'non-living' and 'dead' denote variations in energy and form. In structure the living organism is identical with the non-living—just as the live battery is identical in structure with the dead battery. But what is the essential feature in the living organism upon which structure depends? What is lost in death?

Thirty-five years ago I first attempted to approach this problem by an investigation of the basis of death. Phenomenon after phenomenon associated with death was critically examined and set aside as it was found to be a result rather than a final cause of death.

Studies of the circulation and respiration showed that the changes in these vital phenomena which are present in exhaustion and death are end effects and not primary causes of death. So, too, studies of the chemistry of the blood, while they revealed that the acid-alkali balance of the organism is of vital significance, did not reveal the cause of death.

Resuscitation experiments were performed in which it was found that in normal animals after complete cessation of the circulation for from five to seven and a half minutes resuscitation followed the infusion of adrenalin into an artery; in the course of these experiments the sequence in the return of the various functions and reflexes was observed. But these experiments, important as they were, did not reveal the cause of the cessation of circulation and of the final death which would have followed had not the dose of adrenalin been administered.

By the examination of great numbers of cells under the microscope we found that certain histologic changes were in-

variably present in the cells of the brain and of the liver after death from any but sudden and accidental causes. The nuclear-cytoplasmic relation was disturbed and the semi-permeable membranes were in process of disintegration. That life was incompatible with such a condition of the cells was obvious but was the condition of the cells in itself the actual immediate cause of death?

Later we directed our investigation to the determination of certain physical constants in various vital and lethal conditions. We found that the processes leading to death were always accompanied by a decrease in the conductivity of the brain and of other components of the central nervous system and by an increase in the conductivity of the liver with corresponding changes in the electric capacity of the cells. But certainly a change in the conductivity and capacity could not be the immediate cause of death.

Death may result from many diverse causes—from hemorrhage, from physical injury, from infection, from insomnia, from anesthesia, from asphyxia, from surgical shock, from the excision of certain organs—any of these agents alone may produce death or death may be due to a combination of any of these different factors. Thus in war a soldier may die not as the result of wound alone, or infection alone, or insomnia alone, or anesthesia alone but as the result of a combination of all these various factors. But whatever the cause of death the phenomena of death are identical.

In death the energy characteristic of life is lost—the dead body is in equilibrium. In death the living structures, namely, the cells, are unable to hold their form and structure and inevitably disintegrate. In death the delicate organic molecules such as the fatty-acid chains lose an organizing, binding influence and they too disintegrate.

We propose now to offer new experimental evidence which identifies a form of energy that is lost in death—a form that is capable of constructing the films and of holding together the essential organic molecules.

These researches which have been carried out in the

research laboratories of the Cleveland Clinic Foundation were directed toward finding the relation between electrical potential and oxidation, that is, toward determining whether one or the other is the primary factor in the maintenance of life and whether the loss of one or the other is the essential factor in the production of death or whether both together are primarily essential. Our researches were especially directed toward the discovery of the influence of potential on oxidation; the influence of potential on the form of the living, and by inference on the maintenance of the organic molecules; and to the discovery of the relation of the potential to death.

The results of these researches may be summarized as follows:

1. In animals, plants, and fruits an electric potential exists during life and disappears at death.

2. The potential is varied by insomnia, by anesthetics, by poisons, by hemorrhage, by asphyxia, by change in electrolytic solution, by adrenalin, by injury, by heat and by cold.

3. At the moment of clinical death the potential difference between different organs drops to zero for a few moments; following this, each organ regains its potential for a short time, but finally the potential of all tissues drops to zero, the respiration of the tissues stops, and molecular disintegration sets in.

Are we then correct in ascribing the cause of clinical death to the fall in the potential between the different tissues, and the cause of the death of single cells or of tissue cultures to the fall in the potential on the cell membrane?

Of primary importance was our finding that insomnia by itself alone produces a progressive loss of potential. In our experiments if insomnia was sufficiently protracted the potential declined to zero and the animal died. As the potential approached zero recovery of potential followed a sufficiently prolonged period of sleep and of rest. Of special significance was the fact that after prolonged insomnia the animal did not respond in normal fashion to the injection of adrenalin. (Fig. 1.)

If the molecular structure depends upon an electric strain or potential which also enables the organism to function and

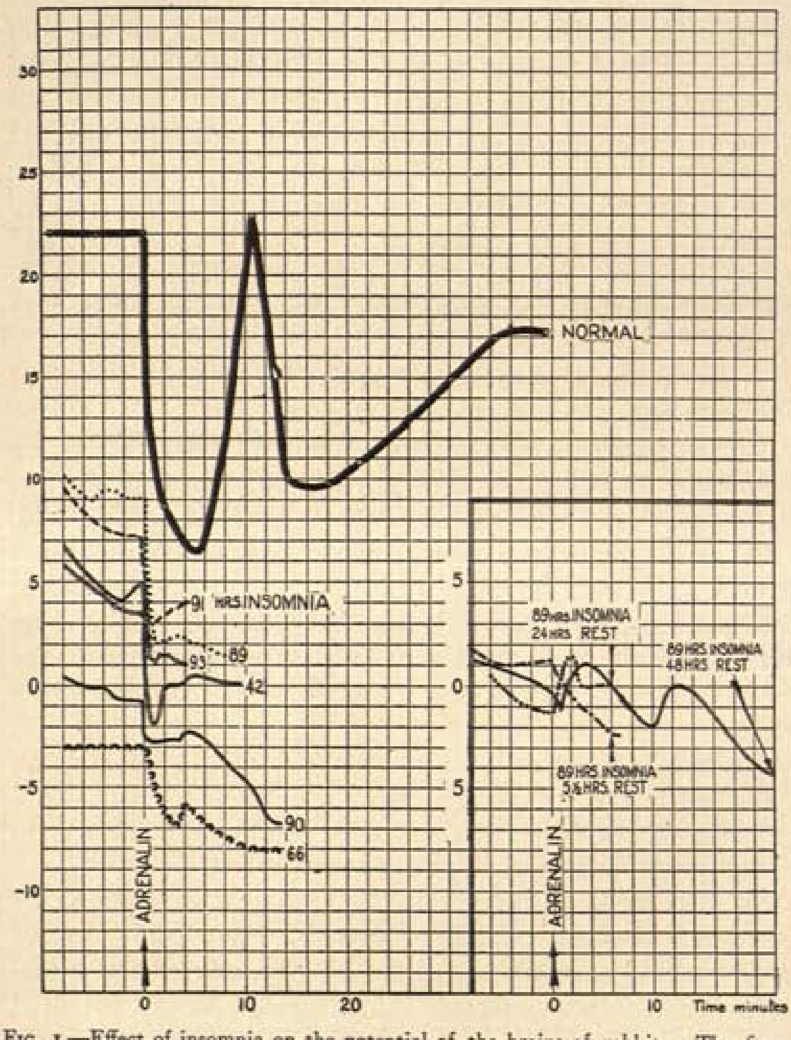

FIG. 1.—Effect of insomnia on the potential of the brains of rabbits. The figures indicate the length in hours of the period of insomnia. Note the lessened response to adrenalin in the 'insomnia rabbits.'

grow, we must find direct evidence therefor. Happily we found such direct evidence in observations of the potential

of an amoeba. Dr. Telkes designed and constructed an electrode which could be inserted into an amoeba and with this electrode she made measurements of its potential. She found that the potential of the amoeba ranges as high as 15 millivolts and that it changes with alterations in the concentration of the electrolytic solution in which it is immersed, with changes in temperature, and when anesthetics are added to the suspending solution. Radiation, adrenalin, and sodium iodid all induce characteristic changes comparable to those seen in rabbits and in dogs. The amoeba was observed under the microscope during the experiments. Here we had our first opportunity of noting under the eye the changes in structure which are produced by these various agents. Of special interest and importance were the effects of changes in potential produced by the direct application of an electric charge which could be varied at will. When the charge was increased the potential rose, and the amoeba became more active. On the other hand when the potential was diminished by introducing a current the direction of which was opposed to that in the amoeba, the amoeba progressively became less active and withdrew its pseudopodia, that is, it rounded up into a quiescent lump, until when the potential reached zero or went over to the negative side the amoeba disintegrated first into larger then into smaller granules and fragments and finally disappeared in the suspending solution. When, however, the potential was lowered by the countercharge and by no other factor nearly to zero and was held there, the amoeba would round up and some granules might even disintegrate; but if at that crucial point when death and disintegration were imminent, the potential was raised by increasing the charge and by nothing else, the amoeba would pass from the resting to the active state, and would again throw out its pseudopodia. (Fig. 2.) This crucial experiment indicates that the fundamental control of the molecular integrity and of the activity of the amoeba—this difference between life and activity and death and dissolution is governed by the change in the electric potential. In our ex-

periments activity, quiescence, death and dissolution were governed completely by the production of variations in the potential alone.

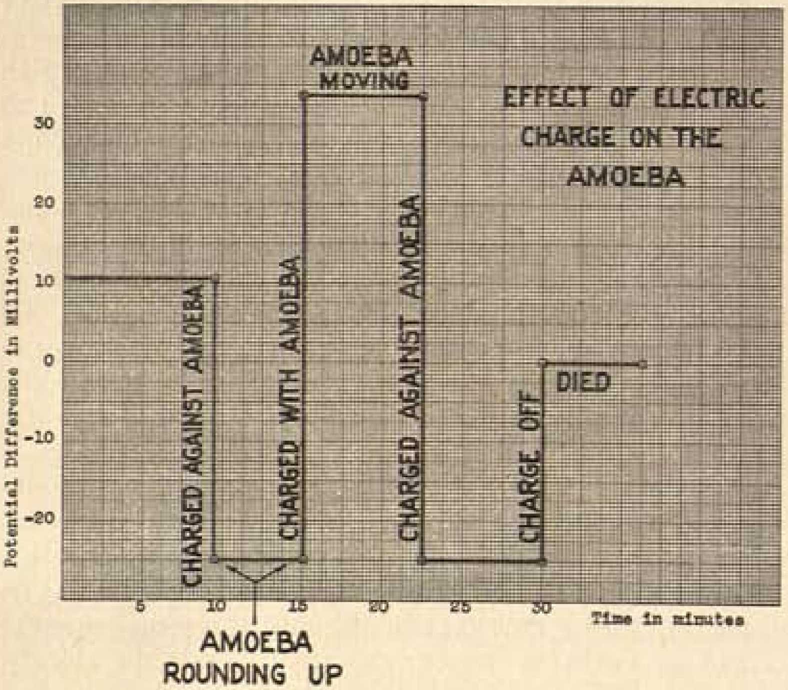

Fig. 2.—Effect on the potential of an amoeba of direct and of counter electric charges.

From this we may infer that the organic molecules that are bound together in the animal organism, the arrangement of crystalloids and colloids, the separation of nucleus and cytoplasm, the maintenance of the molecular organization—we may infer that all these phenomena are manifestations of electric force. Electrical potential is the product of chemical activity and in turn the electrical potential governs chemical activity. These electrical and chemical processes are the governing factors in the production of the phenomena which are characteristic of life. In their absence the organism is dead.

If the organic compounds, structures, etc., in plants and in

animals are created by electric potential and chemical activity, especially by oxidation, then in such intermediate forms of life as fruit the same law should hold.

We therefore extended our researches to an investigation of the phenomena of potential and of oxidation in fruit and we found that every kind of fruit has a potential—the potential of an apple, for example, is about 50 millivolts, and the apple has also a steady respiration—consuming from 3 to 4 c.c. of oxygen every hour.

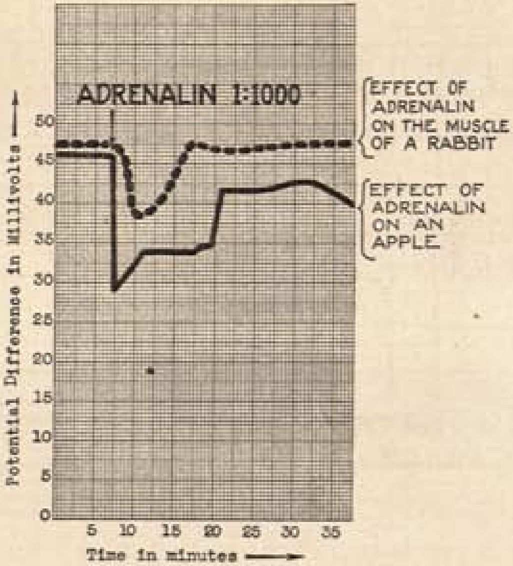

FIG. 3.—Effect of adrenalin on the potential of the muscle of a rabbit and on the potential of an apple.

In our experiments we found that the potential and the respiration of the apple change in the same direction under the influence of anesthetics, and of changes in electrolytic concentration. The administration of adrenalin, changes in temperature, and the exclusion of oxygen affected the apple just as they affect the rabbit and the dog. (Fig. 3.) Anesthetics caused first an "excitant" stage which was indicated by a rise in potential and an increased metabolism; this was fol-

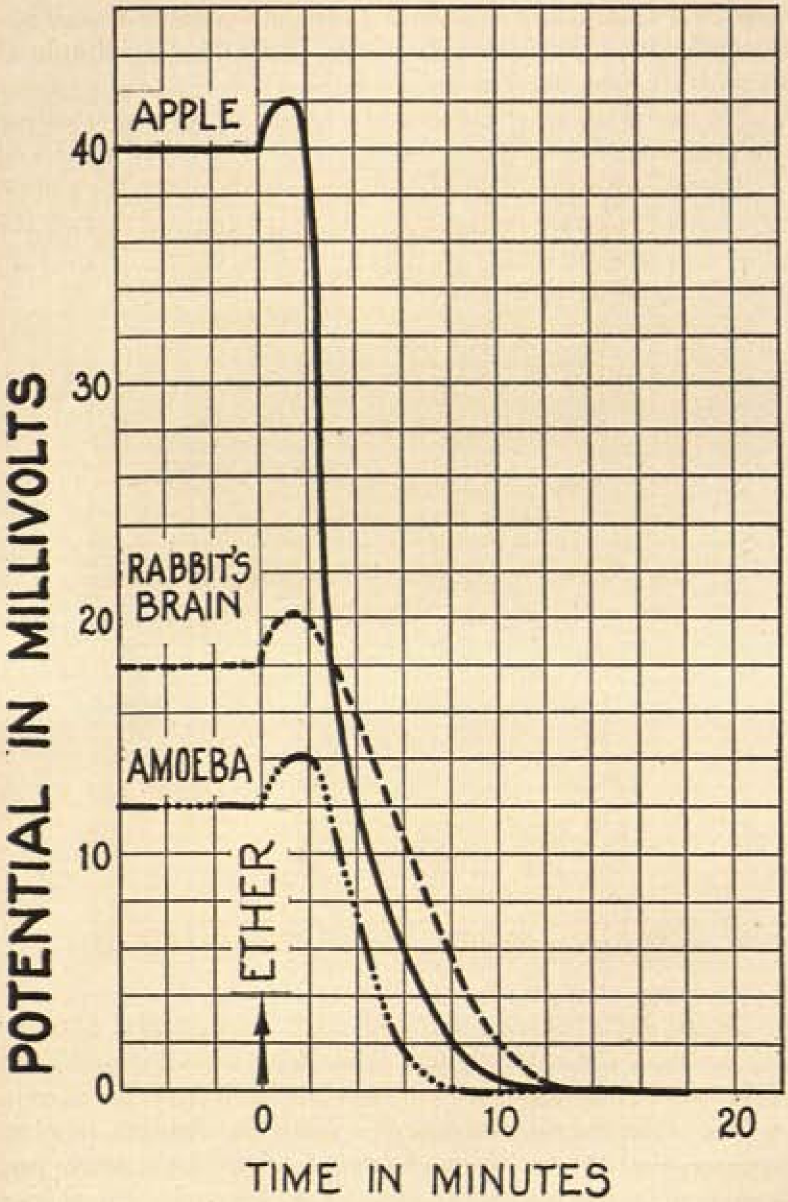

FIG. 4.—Effect of ether on the potential of an apple, the brain of a rabbit, and an amoeba. Note the initial rise followed by the drop in the potential to zero.

lowed by a continuous fall of both the potential and the metabolism to the zero point after which neither potential nor metabolism was again manifested. (Figs. 4 and 5.) Adrenalin caused a fall in potential followed by a rise and the

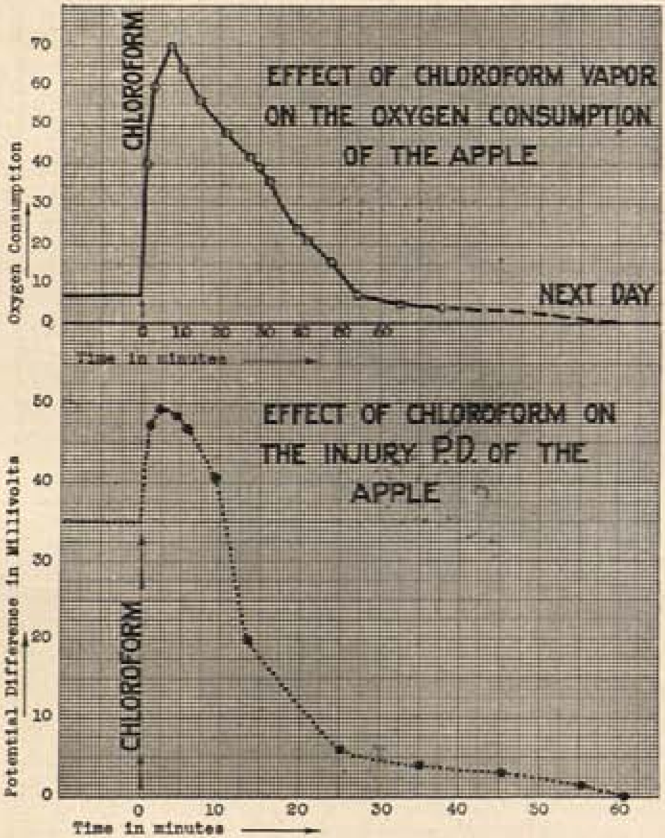

FIG. 5.—Chart showing the effect of chloroform on the potential difference of apples.

respiration of the apple was increased. Changes in electrolytic solutions caused changes in potential which were in conformity with the Nernst formula. (Fig. 6.) When the electrolytic concentration equalled the concentration of the electrolytes in the apple the potential fell to zero. Immersing the apple in oil caused the potential and respiration to fall

to zero and to remain there. Increasing the temperature of the apple caused the potential and respiration to rise together and after an irregular fluctuation at a given height both fell to zero and remained there. (Fig. 7.)

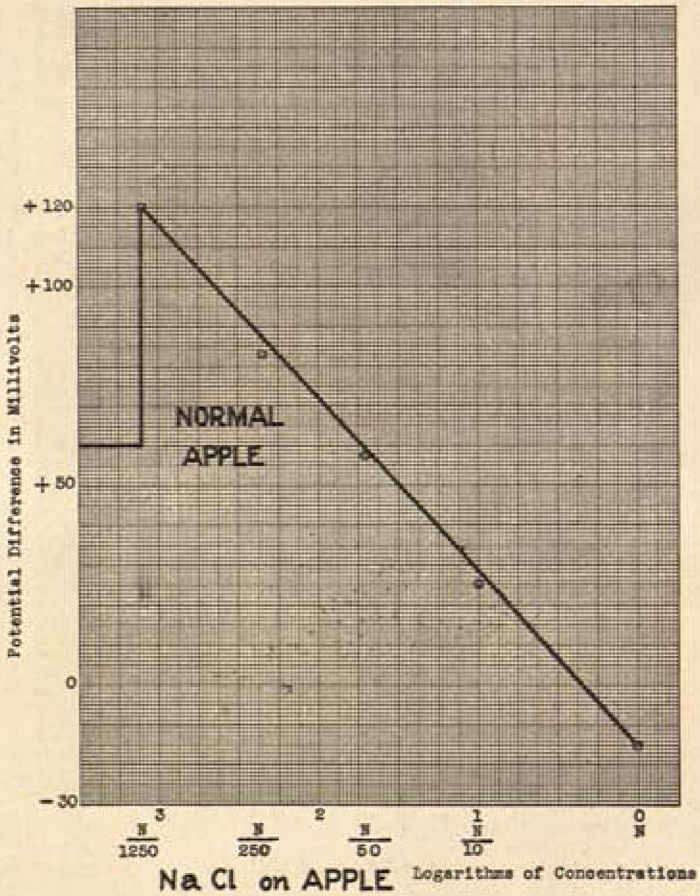

FIG. 6.—Effect of varying concentrations of NaCl on the potential of a normal apple. The curve is plotted in terms of the logarithms of the concentrations according to the Nernst formula. The encircled dots represent the observed values.

In all cases in which the potential was reduced to zero the apple disintegrated just as animals and plants and the amoeba disintegrate when their potential is reduced to zero. A bat-

tery was constructed by arranging halves of apples in series and a potential of over a volt was thus created. (Fig. 8.)

By this study we have demonstrated that the structure of the apple like that of the amoeba is dependent on potential

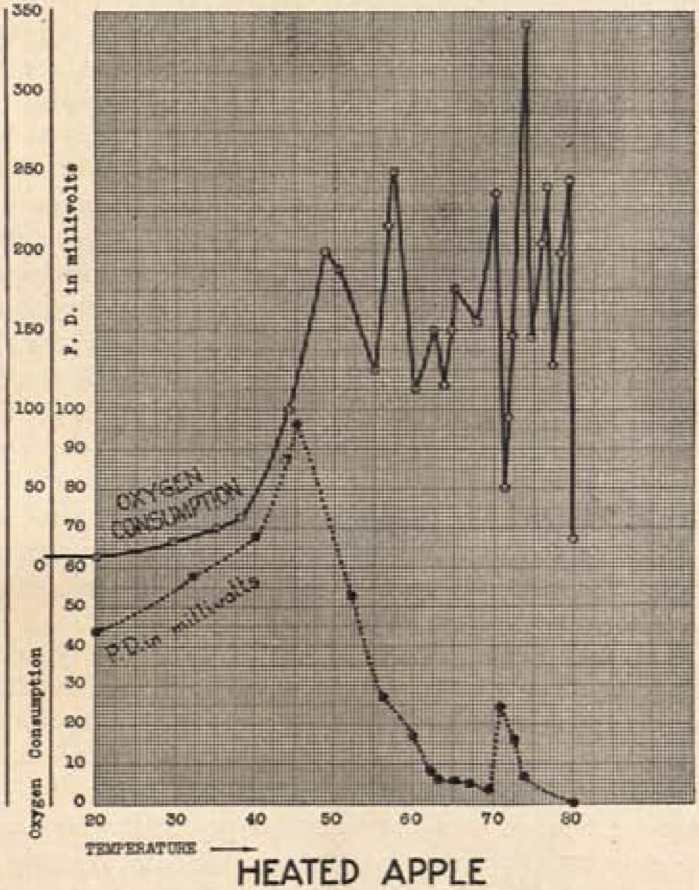

Fig. 7.—Variations in the potential and the temperature of an apple which was gradually heated to 80° C.

and here again we saw the relation between electric strain and the maintenance of the organic structure.

If oxidation is due to a difference of potential and if living cells are concentration cells, then if apple juice were placed on

one side of a celluloid film and distilled water on the other—oxidation and potential should be manifested just as in the apple, amoeba, or rabbit. Such an arrangement was made and as a control another artificial "concentration cell" was set up which was identical with the first except that a hole was punched in the celluloid film in the control cell. Observations of metabolism and of potential showed that the first artificial cell functioned like the apple and the amoeba, that is, it had a potential and it showed respiration; the control cell on the other hand had neither oxidation nor potential.

In brief, then, in a large series of experiments we have found that in animals and in plants and in fruits there exists a potential which has a certain range during life and disappears at death. This potential is dependent on the presence of a semipermeable film, on certain electrolytic concentrations, on water, on temperature, on oxidation, all of which together create the organizing potential. It is the charge on the films of the cells which endows the organism with its selective or adaptive property; oxidation occurs only in the presence of an electric charge and the charge is created by oxidation. Life is a phase of the organization created by electric strain or potential and death is an inert stage in which potential is lost and disintegration is inaugurated.

Or we may define life and death in the following terms: Life may be defined as a potential which is maintained and is varied adaptively according to environmental conditions, this potential being maintained by chemical activity—mainly by oxidation. The loss of this potential is death. The principal difference then between that potential which is life and the potential which is present in non-living systems like concentration cells is that the living potential is spontaneously and adaptively alterable.

In brief then life in the *unicellular* organism is an adaptively changing difference in potential between the cytoplasm and the medium in which it exists, and presumably between the cytoplasm and the nucleus. In the *lowest forms of multicellular organisms* life is an adaptive difference in potential be-

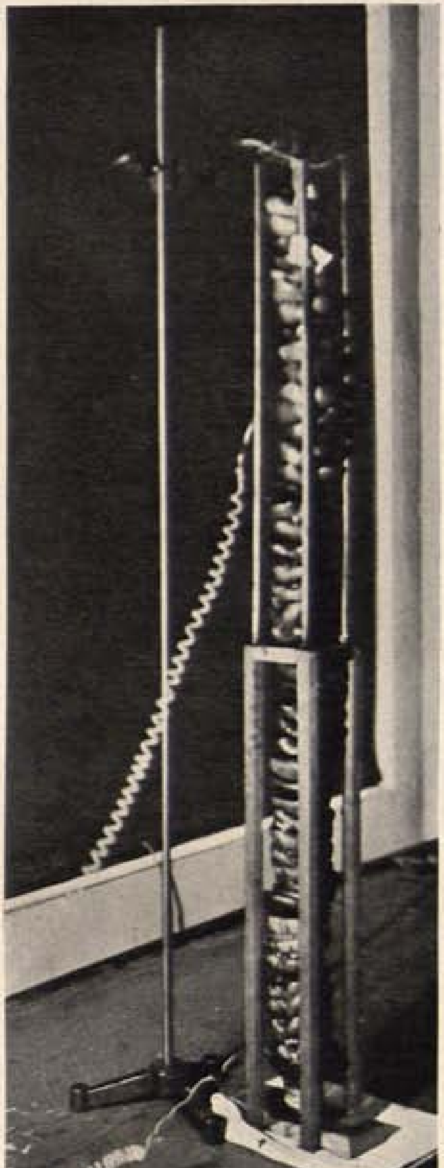

FIG. 8.—An apple "Battery" made up of 50 halves of apples arranged in series. The potential of this battery amounted to more than a volt.

tween the central nervous system and the rest of the organism; in the *higher multicellular organisms* life is an adaptive difference in potential between the brain and the other organs and tissues, especially the liver. The life of an *organ* or *tissue* depends upon the maintenance of a difference of potential between the cells and the intercellular medium and presumably between the nucleus and the cytoplasm of the individual cells of which it is composed. And in unicellular and in multicellular organisms alike death is the absence of a difference of potential—final equilibrium.

THE VEGETATION OF CAMPOS DE JORDÃO, BRAZIL

By JOHN W. HARSHBERGER

(*Read April 19, 1929*)

Geography of Southeastern Brazil

Running parallel to the coast of southeastern Brazil, as it curves from longitude 40° West to longitude 50° West, and between latitudes 15° and 30° South, is a range of mountains known as Serra do Mar, or Coast Range. The highest elevation of this range (2,226.5 meters = 7,323 feet) is in the Serra dos Orgãos (Organ Mountains) near Rio de Janeiro. Serra da Bocaina, the flora of which was described in 1926 by Dr. Bertha Lutz,[1] is a component part of this range west of Rio de Janeiro. Farther inland, separated from the Serra do Mar by the valley of the Parahyba do Sul River through which Estrada de Ferro Central do Brazil (Central Railroad of Brazil) runs from Rio de Janeiro to São Paulo, is the Serra da Mantiquiera, or Serra da Amantiquiera. This inland range culminates in Itatiaia, or Itatiaiassu, the highest peak in Brazil with an elevation variously given as 8,898, 9,400, 9,823, 10,340 feet. The most approved elevation is 2,720 meters (8,822 feet) for this peak. Campos do Jordão is situated on the Serra da Mantiquiera at an elevation of 1,573.808 meters (4,952 feet). São Paulo is 219 kilometers (136 miles) away and Pindamonhangaba, the junction station on the Central Railroad of Brazil from which Campos do Jordão is reached by electric train is distant 46.67 kilometers (28.9 miles).

Vegetational Cross Section of Country

Pindamonhangaba in the valley of the Parahyba River at an elevation of 552.230 meters[2] (1,811 feet) is the station on

[1] *Proc. Amer. Philos. Soc.*, LXV: 27–43, Supplement 1926.

[2] The elevations and distances were copied from the data given on the Station buildings.

the Estrada de Ferro Central do Brazil where connection is made with the electric car of 3 compartments on the Estrada de Ferro Campos do Jordão. The linear distance from Pindamonhangaba to Campos do Jordão is 46.67 kilometers (28.9 miles) with an ascent in altitude of 1,021.578 meters (3,350.775 feet). The electric railroad winds by very steep gradients up from the valley floor to the higher mountain slopes. The station at the foot of the mountains proper is Piracuama at 604.208 meters (1,981.8 feet) altitude, so that in the first 20.445 kilometers (12.69 miles) from the main line an ascent of 51.978 meters (170.4 feet) is made.

The valley floor at Pindamonhangaba is devoted to orange groves and to coffee plantations and between the river and the town is mostly cleared and cultivated land. The cultivated crops comprise bananas, pineapples, sisal hemp, sugar cane, while mangos and papaws appear as fruit trees in the gardens at the edge of the town. In abandoned gardens, *Datura suaveolens* appears, while the castor oil plant, *Ricinus communis* and dandelion *Taraxacum officinale* appear as weeds. Crossing the River Parahyba, a river marsh, or fen, is crossed with a papyrus-like sedge, probably *Cyperus giganteus* Vahl and cattail, *Typha domingensis* Pers. prominent. These river marshes blend with or are transitional to the swamp savannas called in Brazil *campo molhado* and these in turn, as the higher, drier ground is reached pass over into the savanna forest, *cerradão*, where tall feather palms project their feather duster-like tops above the other broad-leaved trees. Large areas of the savanna forest of the flat valley lands have been cleared and burned over, so that in places the *cerradão* has been reduced to groves of trees surrounded by cleared land. When the cleared land has been abandoned, second growth forest has appeared and these areas covered with brush at first are known as *caapueira*, and when covered with a weedy growth as *campo sujo*. Finally when the lower mountain slopes are ascended, a narrow piedmont region is crossed with typical savannas of open country with scattered palms and other tropical trees. Following the terminology

adopted by Wettstein this is probably the *campo limpo* of the natives.

The tropical rain forest, high forest, *matta virgem* (Urwald of the Germans) covers the middle mountain slopes ascending to about 1,161 meters (3,808 feet) to Station Eugenio Lefevre 28.024 kilometers (21.4 miles) distant from Pinmonhangaba. Here cecropias, palms, large tropical trees and associated species form a dense forest with lianes, epiphytes and other growth forms in evidence. Above the tropical rain forest the train enters the region of upland campos (campo alto [1]) with all the rounded mountain tops covered with open grassland. The ravines and valleys are filled with campos forest, so that at an elevation, as one looks across the country, the classification of campos as real savanna is emphasized, for here open grassland alternates with groves of trees occupying the depressions formed where the hill slopes come together. There are isolated trees and groves of trees where the conditions of soil moisture permit their existence. At Campos do Jordão, the highest mountain ascended by the writer was by his aneroid barometer 1,702 meters (5,600 feet), covered with open grassland at the summit. From this elevation the billowy nature of this mountain country with forest-filled depressions was impressed upon the observer.

Climate and Its Influence on Plant Distribution

Schimper in his "Plant Geography Upon a Physiological Basis" (1903), pages 271–273, presents admirably a summary of the climatic conditions which prevail in southeastern Brazil and their influence on plant distribution. The contrast of forest climate and grassland climate is illustrated in crossing the Serra do Mar which opposes its length to the moisture-laden, sea breezes. East of the coastal mountain range is found the tropical rain forest beautifully represented at Alto da Serra between Santos and São Paulo. West of the Serra do Mar, savannas predominate, for the rains have been deposited on the east mountain slopes and the drier winds blow

[1] I have coined this term for the elevated mountain campos.

inland over the Brazilian plateau and the mountain ranges situated less favorably in the interior. As one meteorologist puts it, the southeasterly trade winds are associated closely with the distribution of precipitation through the year. At the time when they are farthest north, conditions of aridity prevail; when they affect more southern latitudes, the season of rains arrive.

Bouillenne has emphasized these influences in the formation of the equatorial savannas of the lower Amazon River. That the middle eastern slopes of Serra da Mantiquiera receive some of the rainfall is evidenced by the existence of a tropical rain forest, while at higher elevations savanna vegetation prevails. This difference of rainfall as influenced by the topography of the country is the cause of the distribution of savannas and tropical rain forests in southeastern Brazil. The seaport of Santos built on land cleared by cutting the high forest has an annual rainfall of 250 cm. (98.25 inches). Raiz de la Serra at the foot of the Serra has a greater rainfall, 280 cm. (110 inches), while Alto da Serra at 800 meters (2,624 feet) above the sea has as much as 336 cm. (132 inches). The rainfall diminishes as we pass inland, so that savannas are formed in the lowlands sheltered from the rain-laden breezes and on the tops of the inland mountains well above the belt covered by the tropical rain forest.

Vegetation of Campos do Jordão

The region investigated by the writer for its vegetation comprised the upland mountain belt above the tropical rain forest near the town of Campos do Jordão which has become noted as a resort for Brazilian consumptives. There are several sanitoria located here, for the salubrious climate at an elevation of 1,524 meters (5,000 feet) has been found beneficial to tuberculous visitors. Two distinct types of vegetation exist here side by side. The rounded summits of the mountains, or domes, are covered with grassland, while the V-shaped depressions between the mountain types are filled with a rather dense forest which may be termed campos forest

(Fig. 1). Warming on page 168 of his book "Lagoa Santa" (1892) gives an excellent sketch drawing [1] (before the time of photographic reproductions) of a Brazilian savanna (campos) with groves alternating with grassland. The grassland and forest groves taken together constitute the campos vegetation, or savanna vegetation, for these upland campos (campo alto) are typical savannas in the accepted phytogeographic use of that term. The two associations of campos vegetation will now be described.

Campos Forest.—The gullies, or ravines, formed by the meeting of the rounded mountain tops, or domes, are filled usually with forest vegetation which conforms in extent to the shape and size of the depressions (Figs. 1 and 6). Sometimes, where the depression is small, a grove of trees will be found. Again where the valley has a longitudinal trend, a narrow forest of considerable length will fill the bottom of the ravine from side to side (Fig. 1). The forest trees and associated species are able to exist because of the soil moisture which comes out at the lower levels of the mountain tops by drainage from above. Sometimes the amount of drainage water is sufficient to cause the formation of springs which give rise to good-sized streams. There is such a stream 3.218 kilometers (2 miles) back of the town of Campos do Jordão, which breaks over the harder, underlying rocks in a series of waterfalls (Fig. 2). Two waterfalls on the same stream were visited by the writer and in the rainy season, the volume of water which goes over these falls must be considerable. The distribution of the trees in these upland gulches has nothing to do with the direction of the slopes. They are indifferent to slopes as to whether they are north, west, south and east. The presence of ground water is the factor which determines the existence of the trees and associated species. Along one of the streams tussock grasses formed a conspicuous association. Two large trees are the most important ones in these upland campos forests. They are most abundant and dominate the association. The pinheiro, *Araucaria brasiliana* (A. Rich) Lamb

[1] This drawing is reproduced in Schimper's "Plant Geography," page 363.

(Figs. 3 and 6) and the pinheirinhas, *Podocarpus Lambertii* Klotzsch (Fig. 4) are the two most important species.

Araucaria brasiliana is a striking evergreen tree with its whorls of branches arranged in ascending series each branch ending in a brush of stiff, dark-green leaves (Fig. 3). The young trees are compact and spire-shaped, while the old trees become flat-topped. The sharp-pointed, scale leaves of this conifer persist for a long time on the main trunk and impart a rough surface to it. A tree may be 10 cm. (4 inches) in diameter and its trunk may be covered with the old scale leaves, the surface of which may be covered with epiphytic lichens. The cones, when mature, break up like in the northern firs (*Abies*), so that only a few basal scales and the pointed cone axis remain. The seeds are quite large with straight embryo having two strap-shaped cotyledons applied together and surrounded by an abundant reserved food tasting like raw chestnuts. A suspensor can be located at the pointed end of the seed. Only the older trees are destitute of the lower branches, while above, they become umbrella-shaped. These trees as a rule do not crowd together to produce pure growths, but they are scattered about so that the crowns of the old trees can assume their characteristic form. In this scattered growth they differ from the same species in the Brazilian State of Parana where extensive pure forests are formed with the trees massed together. On the savannas of Campos do Jordão they are abundant enough, however, to give stamp to the campos forest which might be termed appropriately an *Araucarial* (Figs. 3 and 6), because of the dominance of this one species of tree. Sometimes these trees grow singly on the open campos, but if they are found in such situations the forest groves from which they have been seeded are not far away. These sentinel trees are sometimes of large size. *Araucaria brasiliana*, which grows in these mountains at high elevations, forms very extensive forests in southern Brazil where the Serra do Mar gradually subsides, but where the latitude produces lower temperatures, which correspond to those of the higher mountains farther north.

FIG. 1.—View toward Southeast from Highest-mountain dome at Campos do Jordão, Brazil, July 23, 1927. Note grassland on rounded mountain tops and the forest groves in the depressions.

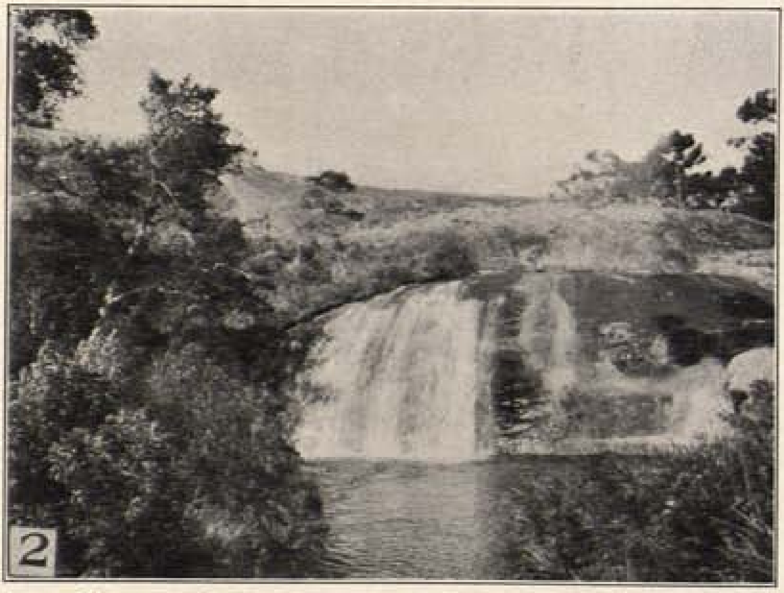

FIG. 2.—Upper waterfall of stream draining the upland campos, Campos do Jordão, July 23, 1927.

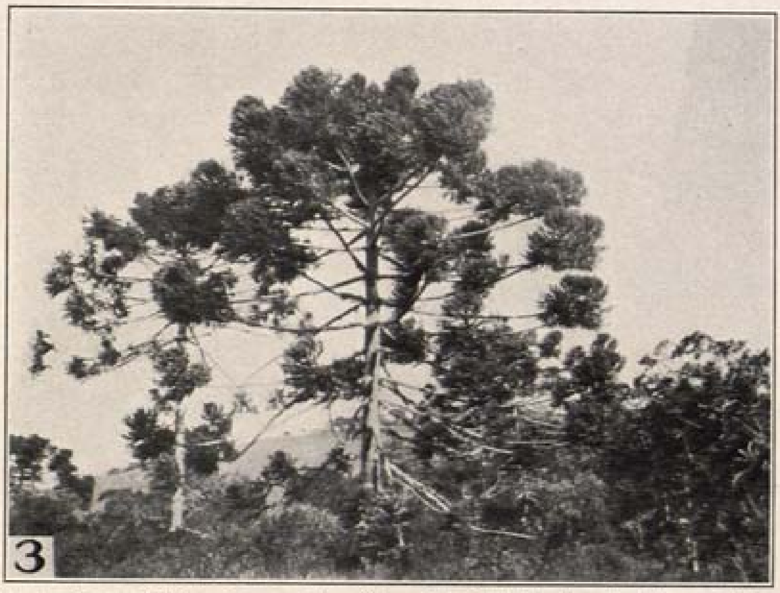

Fig. 3.—Large pinheiras *Araucaria brasiliana* with its limbs draped with gray lichens (*Usnea*) in valley bottom with tussock grasses. Campos do Jordão, July 23, 1927.

Fig. 4.—Large pinheirinhas, *Podocarpus Lambertii*, in campos forest. Campos do Jordão, July 23, 1929.

FIG. 5.—Ant hill invaded by two small plants at edge of campos forest. Campos do Jordão, July 23, 1927.

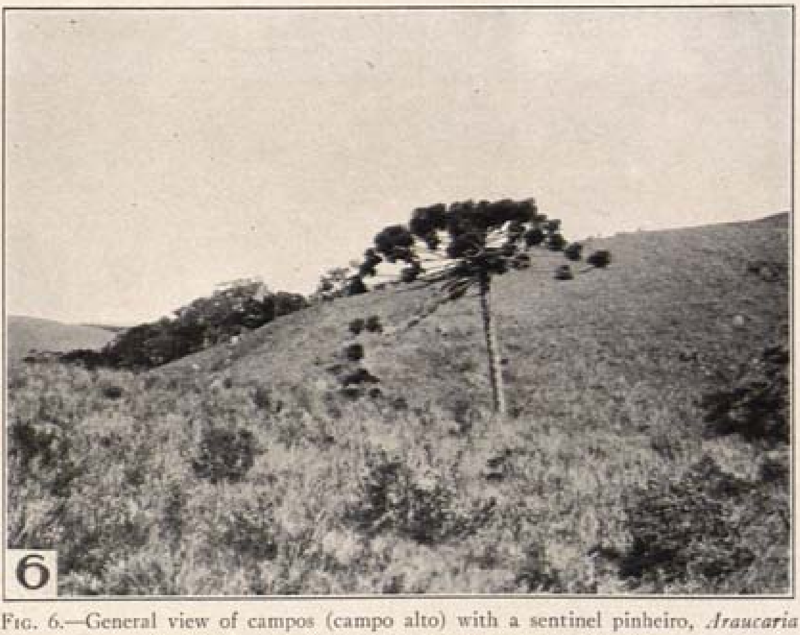

FIG. 6.—General view of campos (campo alto) with a sentinel pinheiro, *Araucaria brasiliana*. Campos do Jordão, Brazil, July 23, 1927.

The pinheirinhas, *Podocarpus Lambertii* Klotzsch (Fig. 4) is also a fine tree but with a habit like a juniper. It is round-headed and dark green with its numerous, rather broad, evergreen leaves. The trunk and limbs of these trees are covered with various epiphytic bromeliads, orchids, ferns, mosses, lichens, etc., so that a gray tone is given to the forest where these airplants inhabit the pinheiro and the pinheirinhas. The following epiphytes collected by me on the trees in the campos forest at Campos do Jordão on July 23, 1927, between 1,585.36 meters (5,200 feet) and 1,646.34 meters (5,400 feet) are noteworthy.

Lichens[1]

- *Anaptychia leucomelaena* Messel
- *Megalosporia taitensis* (Mont.)
- *Parmelia cetrata* Ach (?)
- *Parmelia reticulata*
- *Parmelia rudecta* Ach
- *Parmelia trichotera* Hoe emed (or tree orchids)
- *Usnea angulata f. goneodes*
- *Usnea articulata* Linn
- *Usnea cinchonarum* A. Zahlbr
- *Usnea gracilis* Ach
- *Usnea longissima* Ach
- *Telaschistes acromela* Wain

According to F. C. Hoehne the crustaceous lichen, *Chiodecton sanguineum* paints the smooth bark of trees bright pink with its extensive growths. The epiphytic mosses collected by me comprise *Leptodontium citrinum* Hamp and *Macromitrium* sp. The polypody fern, *Polypodium typicum* Fee, was common on trees in the campos forest. A pepper plant, *Peperomia trineura* Miq. was collected as an epiphytic flowering plant. A species of *Mimosa* was noted as a small tree in these forest groves and well down in the stream valley below the lower waterfall grew a tall tree fern *Cyathea Schanschin* Mart. collected at Campos do Jordão by Dr. F. C.

[1] Determined by Miss A. Lorrain Smith, British Museum of Natural History, London.

Hoehne and illustrated by him in two figures, one with its trunk covered with an hymenophyllaceous fern.

Grassland. The rounded mountain tops, or domes, fully exposed to the winds, and where the soil is relatively dry, are covered with grasses and associated species (Figs. 1 and 6). The prairie aspect of these upland grasslands is characteristic. It is a short grass vegetation on a red soil baked hard during the dry season. One of the most marked features of the open campos were the large conical ant hills over 1 meter (four feet) tall (Fig. 5). As long as these ant hills are tenanted by the ants, they are kept free of a plant covering, but as soon as the vigor of the ant colony begins to deteriorate, or when the hill is deserted, then their outer surface is invaded by various plants until they may be completely covered and thus hidden from sight (Fig. 5).

On July 23, 1927, when the vegetation was investigated by the writer, the plants were mainly brown and in a dried condition. A sufficient number of species were found in flower, so that they have been identified by Mr. E. P. Killip of the United States National Museum. The following grasses were collected on the open campos at Campos do Jordão: *Aristida torta* (Nees) Kth., *Andropogon ternatus* (Apr.) Nees, *Andropogon virgatus* Desv. As associated species the following plants were found on the dry, treeless grassland: *Plantago tomentosa* Lam., *Microlicia isophylla* D.C., *Clidemia rubra* (Aubl.) Mart., *Eryngium paniculatum* Cav., *Inulopsis scaposa* (D.C.) O. Hoffm., *Baccharis genistelloides* (Lam.) Pers., *Baccharis erioclada* D.C., *Baccharis lundii* D.C. and *Achyrocline vargasiana* D.C. *Baccharis genistelloides* (Lam.) Pers. was one of the most common and striking herbaceous plants in flower and fruit at the time of the writer's visit. A perennial compositous plant about 46 cm. (18 inches) tall, it had a green stem with broad, flat, projecting wings and small creamy white heads of flowers. The small achenes provided with a copious pappus gave the whole plant a cottony aspect. The occasional presence of a single large tree of *Araucaria brasiliana* on the open grassland is hard to explain (Fig. 6).

General Aspects of Campos Plants

Professor Eug. Warming, the Danish ecologist, in his earlier years spent several years in Brazil and as a result of his botanical investigations there published a number of important botanical monographs, such as his one on the family *Podostemaceae* and his "Lagoa Santa" (1892). The latter book gives a detailed account of his studies of the campos vegetation of Lagoa Santa (1821). The latter book gives a detailed account of his studies of the campos vegetation of Lagoa Santa in the Brazilian state of Minas Geraes. Later a synopsis of his investigation and that of other ecologists was published in his "Œcology of Plants" (1909) pages 296–297. Briefly with the exception of a small percentage campos plants are perennials. The grasses are cespitose with narrow, stiff, hairy leaves. The perennial herbs, as well as many of the undershrubs and shrubs develop under the soil tuber-like, woody bodies (*xylopodia*) which seem to be a fusion of root and stem (mostly stem) and from these structures numerous unbranched, or feebly branched shoots arise. Plants with bulbs, tubers and succulent parts are wanting, or rare. The leaves of the dicotyledons are xerophilous, sometimes stiff, often of small size. There is an almost complete absence of ericoid and pinoid types. The lack of a collection of plants representing the complete annual flora of the region will prevent a plant spectrum of growth forms being made.

The general physiognomy of the campos vegetation is a result of the general lack of rain in June, July and August, and the high percentage of evaporation throughout the year.

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(*Abstract*)

MEAN SEALEVEL STUDIES IN NEW YORK WATERS

By DOUGLAS JOHNSON

(*Read April 20, 1929*)

At an earlier meeting of the Society the writer advanced the theory that along an irregular coast mean sealevel is an irregularly warped surface, and a surface which is extremely sensitive to changes in the form of the shore, varying in vertical position with alterations in the shape and position of bars, channels, and other shore features. To test this theory the Committee on Shoreline Investigations of the National Research Council with the generous coöperation of the United States Coast and Geodetic Survey, the Department of Docks and the Department of Plant and Structures of New York City have for more than a year been prosecuting tidal studies in Jamaica Bay and New York Harbor at four tidal stations established for the purpose and connected by lines of precise level. The Jamaica Bay region was selected because the small size of the Bay, the relatively large breadth and depth of the inlet, the small amount of land water entering the Bay and other factors made it seem probable that the distortion of mean sealevel in this locality would be very slight. If it could be shown that under conditions relatively unfavorable to the distortion of mean sealevel the fact of distortion could nevertheless be recognized and measured, we would have adequate proof that at many points along the Atlantic coast distortions of mean sealevel of much greater amounts must be expected, and that observed changes in mean sealevel cannot be attributed to vertical movements of the land or to general changes in the position of sealevel until the possibility of local fluctuations in mean sealevel resulting from shore changes has been definitely excluded.

In a paper published in the Proceedings of the Society for 1927 it was predicted that as a result of the Jamaica Bay studies the differences in sealevel within the Bay would be found to be "very small, at most a very few inches and possibly only fractions of an inch." The investigations have revealed the fact that mean sealevel within the Bay is everywhere higher than mean sealevel at Fort Hamilton, on a more exposed part of the shore; that the plane of mean sealevel within the Bay is warped, being highest at the northeast and lower at the south and west; and that the elevations of mean sealevel at the three tidal stations are 2.04 inches, .72 inch and .84 inch respectively above mean sealevel at Fort Hamilton. That the predicted values should be equalled or exceeded by the observed values gives one added confidence that distortions of the mean sealevel plane may safely be inferred from a consideration of the physiography of a given shoreline.

An analysis of tide gauge readings at Fort Hamilton extending over a third of a century shows that the average mean sealevel for the first five year period of the last thirty years of observations differs from the average mean sealevel of the last five years by only .01 foot. This result supports the theory, previously presented before the Society, that there has been no appreciable progressive subsidence of the coast in modern times.

OLD-WORLD PREHISTORY IN RETROSPECT AND PROSPECT

By GEORGE GRANT MacCURDY

(Read April 18, 1929)

The birth of prehistory as a science may be said to have taken place when C. J. Thomsen of Denmark laid the foundation for a system of prehistoric chronology. With the coöperation of a number of Danish and Swedish scientists including Forchhammer the geologist, Nilsson the zoölogist, Worsaae, and others, Thomsen succeeded in establishing on a scientific basis a relative chronology for prehistoric time. Thomsen began to make use of this system in 1830, but he did not publish it until 1836.[1] His researches gave confirmation beyond the shadow of a doubt to beliefs which had been expressed by men of thought and vision even before the beginning of our era. Thomsen's classification provided for a sequence of cultures representing three ages: Stone, Bronze and Iron. His system, applied to the collections in the National Museum at Copenhagen, of which he was director, was soon accepted in Scandinavia and throughout the world.

Prehistory as a science therefore will very soon be one hundred years old. Although progress has been comparatively rapid during the last hundred years, the science is still in an early stage of development. The reasons why the scientific study of man's remote past has had such a tardy beginning are not far to seek. Man must perforce be his own examiner and classifier and sit in judgment on the results of self-investigation, a doubly difficult task made all the more difficult through pitfalls of myth, prejudice and superstition.

Prehistory is a subject of three dimensions—two geographic and one geologic. From the latter we gain some

[1] *Ledetraad til nordisk Oldkyndighed.*

idea as to the length of time that has elapsed since man first appeared on the earth; from geography we hope to learn something of the distribution of prehistoric cultures and races over the face of the earth; lastly the geographic and geologic data combined should make it possible to determine approximately where man's first appearance occurred. It is obvious therefore that before the science of prehistory could be developed, it had to await the prior development of our knowledge of geography and geology. With the extension of geographic knowledge through voyages of discovery, there were revealed hitherto unknown races in a primitive stage of culture. A study of their cultures and physical types afforded very important means of comparison with prehistoric races and cultures; so that both ethnology and prehistory have been the gainers from geographic research.

Still other sciences that had to be developed in advance of prehistory were human and comparative anatomy and paleontology, without a previous knowledge of which, the uncovering of fossil human remains would have been worse then futile; for these would never have been preserved and just so much valuable data would have been forever lost. It was a bit of great good fortune that the accidental discovery of the human bones at Neandertal did not take place during the middle ages. We shall never know how much of the record of fossil man was uncovered and destroyed prior to the beginning say of the nineteenth century.

Think for example of the precious relics that were destroyed when, what is now known as the *abri du Château*, a Magdalenian rock shelter at Les Eyzies (Dordogne) was almost emptied in order to prepare the foundation for the medieval château, itself in ruins until restored a few years ago as a museum of prehistory. Perhaps an even greater loss of prehistoric material was incurred when the floor deposits of the great cave of George d'Enfer, near Les Eyzies, were removed and in part made use of in the manufacture of gunpowder.

Whatever may have been the shortcomings of the prehis-

toric races, their stages of culture were such as not to require any great drafts on the natural resources. Great builders are of necessity great destroyers; the earliest races were neither. The prehistoric period was therefore one of slow accumulation of records and their preservation through Nature's own processes. It is only during historic time that the face of the earth has been materially changed through human activities: tilling of the soil, deforestation, mining, quarrying, the making of roadways and great cities. In these operations, deposits containing precious records of the past have been depleted with resultant wholesale destruction of these records until about one hundred years ago. Even now where deposits are exploited on a grand scale, or in regions where a knowledge of prehistory is limited to the very few, there is a certain percentage of loss. The need for trained field workers in prehistory was never greater and to fill this need is one of the chief purposes of the recently founded (1921) American School of Prehistoric Research.

The evidence thus far gleaned points to the Old World as the stage on which the first acts of the human drama were played. Let us examine for a moment the stage. It was ample in size; the greater part of the land mass lies north of the equator and in the hemisphere which suffered least from the recurring advances of the ice during the Glacial Period, thus leaving to man a more ample stage for the great drama of physical and cultural evolution than he would have had in the western hemisphere. Our story will therefore deal primarily with the prehistory of the Old World.

There is every reason to assume that the cradle of the human race was not only somewhere in the Old World but also north of the equator. Was it in that part now known as Europe? It is too early to answer this question definitely. We are however in a better position to discuss the prehistory of Europe than that of any other section of the Old World because more work has been done there than anywhere else.

Prehistory is unthinkable without a chronology, but its

chronology is of a sort to which the finer units of the time scale—such as days, months, and even years—are not applicable. Its chronology is measured by year units to be sure, but used in mass rather than singly; and when figures in terms of years are given, they must be considered as approximations—as is the case when dealing with the Ice Age or with geologic time. This fact however does not reflect on the validity of prehistoric chronology, or on its scientific value. Where we have to deal with big units of time, a difference of opinion of even thousands of years is no very serious matter. In consulting various authorities, therefore one should not be surprised to find chronological disparities.

Primitive peoples have a vague appreciation of their own prehistoric past, the vagueness being due to the fact that their knowledge of it is based on myth rather than on observation of the only records which are capable of throwing any light on the prehistoric. Even after a people enters upon a true historic era, its ideas concerning the era lying back of the historic often continue to be vague for a considerable lapse of time. This is especially true of the Old World peoples who were the first to appear on the historic horizon. It has taken some of them at least five thousand years to rid themselves of the yoke of myth and superstition; and the science of prehistory has done more in the last hundred years to rid the world of these than perhaps any other one agency. Both credit and honor therefore should go to those whose exceptional powers of discernment have combined to make prehistory what it is today.

The course of progress has always been and will continue to be thought out by the gifted few. The rate of progress has always depended and will continue to depend on the ability of the many to profit by the achievements of the few. An individual may achieve something wonderful, but if this fails to register, to take effect in the minds of the many, it falls little short of an abortive attempt so far as results are concerned. Thus many an idea fraught with great possibilities has died with its author. Then there is the idea that passed

into a state of coma upon the death of its originator and was later luckily resuscitated by one of the gifted few. In both cases the time was not wholly ripe for launching the idea. There is a third class of new ideas whose originators are fortunate enough to live to see the full fruition of their thoughts. It follows therefore that the time when a great event happens, a great idea is launched, is not so important so far as progress is concerned as the time when the event or the idea takes effect and begins to bear fruit.

We cannot name any concrete examples of the first class for in it belong the "mute inglorious Miltons" of whom the number is legion. To the second class belong the Freres, the Mendel's, the Sautuolas, who did not live to see the sprouting in other minds of the seeds they had sown. As examples of the third and more fortunate class we may cite such names as Boucher de Perthes, Lartet, Darwin, who lived to see the fruition of their labors.

It mattered little to men living in the darkness of the Middle Ages that some 850 B.C., Hesiod spoke of a time when bronze had not yet been superseded by iron; that Lucretius (98–55B.C.) had said: "The first weapons were the hands, nails, teeth, stones, branches broken from the trees; to these were added fire, but a long time elapsed before the discovery of bronze and iron. Bronze was the precursor of iron." The man who discovered in 1690 the first paleolithic hand ax of flint at a considerable depth under what is now the heart of London and recognized it as a prehistoric artifact, fared little better at the hands of his contemporaries and immediate descendants. The incident had been forgotten when in 1797 John Frere made a similar discovery but on a grander scale at Hoxne in Suffolk. And still the time was not ripe. The significance of John Frere's discovery failed to take effect. It remained for Boucher de Perthes with a persistence and a vision far beyond the ordinary to convince the scientific world of the reality of a prehistoric past, long antedating anything that had, until then, been conceived of.

The final success of Boucher de Perthes did much toward

making it comparatively easy for Edouard Lartet only a few years later to prove by discoveries in cave deposits that the dawn of the fine arts occurred during the last glacial epoch. It was the head of a bear engraved on a piece of deer horn from the cave of Massat (Ariège), which first attracted Lartet's attention. Then followed in quick succession similar discoveries by him at Bruniquel and in the Vézère valley. The finds made at Bruniquel were in a cave owned by Vicomte de Lastic, who decided to take over the digging himself. He found a number of examples of cave art which were offered to the Louvre. Unfortunately for France, Longpérier, curator of antiquities, failed to appreciate their significance so they went to the British Museum instead. Switzerland lost to Germany the browsing reindeer from Kesslerloch for similar reasons. These things would not have happened if Lartet's views had only gained credence a little sooner.

The examples of cave art discovered by Lartet and others in his time were all portable objects found in the floor deposits of caves and rock shelters. He died in 1871 ignorant of the existence of paleolithic mural art in the form of polychrome frescoes, drawings, engravings and figures in relief. Eight years later Sautuola discovered the great group of incomparable frescoes on the ceiling of the cavern of Altamira; but the results of his epoch-making discovery did not register until nearly a score of years after his death.

Thus we see that the same old struggle, through which chipped flints had to pass before their great antiquity was recognized, was repeated in the case of the art of the caveman As with flints and fine arts, so also with the fossil human bones which went unrecognized all too long. The finding of the Gibralter skull in 1848 did not cause a ripple. The Neandertal skeleton discovered in 1857 fared much better. This was partly due to the timeliness of Schaaffhausen's first paper on the subject appearing as it did in 1858, coincidently with the memorable meeting of the Linnean Society in London, at which was presented the joint communication of Darwin and Wallace "On the Tendency of Species to form Varieties; and

on the Perpetuation of Varieties and Species by Natural Means of Selection." The next year (1859) witnessed the appearance of Darwin's "*Origin of Species*" and the establishment of the authenticity of Boucher de Perthes' discoveries in the valley of the Somme. The backwash caused by the almost simultaneous launching of these four big scientific events revealed anew and in a different light the Gibralter skull. Seldom have so many great events synchronized and contributed their combined weight toward giving impetus to a scientific movement. This movement was given additional impetus by Lartet's discovery of cave art the following year (1860). These five events of overwhelming importance occurring as they did within the short space of three years has set a record, of which the nineteenth century may well be proud.

There was however one drawback in connection with the finding of the Neandertal skeleton which caused some doubters still to remain in doubt. There was just a bare chance that it might be only a pathological specimen and not a species of fossil *Homo*. Then again no artifacts were found in association with it. Even if these had been found, there would have been no one prepared to interpret their meaning and give them their proper place in the scale of culture sequence.

It was not until 1886 that human skeletons of the same fossile race were found in situ and associated with artifacts. The latter turned out to be like those found in 1862–63 by Lartet and Christy in the lower deposits at the rock shelter of Le Moustier (Dordogne). To these de Mortillet had in the meantime (1869) assigned a place in his table of relative chronology, viz., the Mousterian Epoch. Thus at Spy, Belgium, in 1886, Fraipont and Lohest, with de Puydt, found two skeletons of the same race as Neandertal and in association with Mousterian artifacts. But the race had already been christened *Homo neandertalensis* by King (1864), and to the epoch in which he lived de Mortillet (1869) had already given the name *Mousterian* based on the artifacts from Le Moustier. This

is the reason why the culture left by Neandertal man is not called Neandertalian and why the race is not called *Homo mousteriensis*.

So much is being said about Neandertal man that it is time those who use the term should come to an agreement as to how it should be spelled as well as pronounced. The original spelling was with an "h" although the "h" was never pronounced by the Germans. Now that the German *Reallexikon der Vorgeschichte* has set its seal of approval on a practise already in vogue among both German and non-German prehistorians, of leaving out the "h," would it not be wise for English and American authors to follow suit, thus avoiding the danger of pronouncing the last syllable "thal" with an "h" sound, instead of "tal" as it should be pronounced as well as spelled?

There are a few words in the nomenclature of prehistory, about which there should be agreement as to pronunciation. I refer to the three words Chellean, Acheulian and Cro-Magnon. In the first two, the "c" in "ch" has an "s" sound in French. In converting these words into English, it is proposed to retain the French sound by pronouncing the words as if they were spelled "Shellean" and "Asheulian" respectively, instead of "Tchellean" and "Atcheulian," which one sometimes hears from the lips of English-speaking persons. Then there is the word Cro-Magnon. The French pronounce the last half of this hyphenated word as if it were spelled "Manyon." There seems to be no valid reason why the English pronunciation should not be the same as the French instead of employing the hard "g" sound.

One of the big problems in European prehistory is the correlation of human cultural and skeletal remains with the various phases of the Ice Age. Glaciologists have combined with prehistorians with the result that at least a good beginning has been made toward referring stations of the Old Stone Age to the various glacial and interglacial epochs. In brief Reid Moir and Breuil, who have recently given much attention to this subject, would correlate the Pre-Chellean

and Chellean with the Günz-Mindel, or the first interglacial epoch, and the Acheulian with the Mindel-Riss (second interglacial). This would place the long warm phase of the Mousterian Epoch in the third interglacial or Riss-Würm Epoch, and the cold Mousterian, Aurignacian, Solutrean, and Magdalenian in that which remains of the Ice Age, namely the advance, maximum and retreat of the Würm glaciation.

If Breuil is right man began to live in caves much earlier than was supposed; for he would refer the industry at the base of the relic-bearing deposits from the grotte de l'Observatoire in Monaco to the Pre-Chellean Epoch. Even Boule who is ultra conservative would class the oldest industry from the Observatoire station as Chellean.

It should not be inferred that man lived in Europe only during interglacial epochs; even during the maximum of each of the four glaciations there was always room left for man to maintain a foothold. Breuil definitely refers the Paleolithic station of Coomb Rock, England, to the Riss Glacial Epoch. For the culture of this Epoch and the succeeding Riss-Würm Interglacial, he would coin a new term "Levalloisian" to be intercalated between the Acheulian and the cold Mousterian. Breuil considers the Lower Boulder Clay of England to be the product of the Mindel glaciation and the Upper Boulder Clay to be that of the Riss glaciation.

The Paleolithic and Mesolithic cultures of Europe have been more completely studied than have those of the Neolithic and the Age of Metals. This is partly due to the fact that the problem has been simpler. In spite of difficulties encountered, our knowledge of the Neolithic is yearly becoming better defined. This progress is due in large measure to the Scandinavians, the Slavic races, the Swiss, and the Germans, although credit must be given to practically every other nation for a share in it. In spite of all that has been done there is still no well-defined chronology of the New Stone Age that will apply to anything more than restricted regions. It remains to be seen whether such a chronology and terminology can be determined that can cover satisfactorily the whole of

Europe. No one has as yet arisen to become the arbiter of Neolithic chronology; perhaps the time may not yet be ripe for such a consummation. In this field the geologist and paleontologist can be of little service to the prehistorian. Besides, the very complexity of the problems to be solved has put a damper on initiative. What is true of the Neolithic is also true of the Bronze and Iron Ages. Enough however is being done to prove that the problems are not insoluble. The intensive work that is now going on will prepare the way for works of a comparative nature and eventually make possible a wider correlation of the culture of one region with those of its neighbors.

One of the hopeful signs of the times testifying to an abiding interest in prehistory especially of Europe and the Old World in general is that three institutions have recently been incorporated to do work especially in this field. I refer to the *Institut de Paléontologie Humaine* in Paris, the *Urgeschichtliches Forschungsinstitut* at Tübingen, and the *American School of Prehistoric Research*. All three are needed to supplement the activities of pre-existing universities, museums, societies, etc. It is also perhaps worthy of mention in this connection that, at the meeting of the British Association for the Advancement of Science in Glasgow last September, of the thirty papers which were read before Section H, twenty-three or over three forths, were on the subject of Old-World prehistory and fifteen, or exactly half, on European prehistory.

An even better indication of the lively interest taken in European prehistory is the ever-increasing volume of the current literature, both technical and popular, on the subject. Perhaps the most significant example is the *Reallexikon der Vorgeschichte*, the first volume of which appeared in 1924 and of which the first two numbers of volume XIV have just been issued from the press. The first volume of IPEK—*Jahrbuch für Prähistorische und Ethnographische Kunst* (Leipzig) appeared in 1925; the *Vorgeschichtliches Jahrbuch* (Berlin) in 1926; and *Antiquity* (Gloucester) in 1927. Max Ebert is editor of the

Reallexikon and the *Vorgeschichtliches Jahrbuch*; Herbert Kühn of IPEK; and O. G. S. Crawford, of *Antiquity*. The first memoir in the series entitled *Archives de l'Institut de Paléontologie Humaine* appeared in 1927 and since then three more memoirs of the series have been issued.

A number of creditable works on European prehistory have appeared during the past twelve months in various languages, especially English, French and German, so that the general reader's opportunities for keeping well informed on the subject were never better.

The major and pressing problem before the prehistorian is to increase our knowledge of the prehistory of Asia and Africa in order that correlation of Old-World Prehistory as a whole may be achieved. To this end, the American School of Prehistoric Research has obtained permits and is already exploring and excavating jointly with the British in Iraq, Palestine and Transjordania.

The campaign of last autumn was highly successful. The area explored was the region of Sulaimani, about 160 miles miles northeast of Bagdad. Many caves and rockshelters were located. A small cave at Zarzi proved to be exceedingly rich in remains belonging to the Aurignacian Epoch (Paleolithic), the principal types being exactly comparable with those from Aurignacian stations in the Danube valley, lower Austria, and south central France. Another interesting feature is that at the top of the deposit this typically Aurignacian culture grades off into the microlithic Tardenoisian (Mesolithic). In other words, remains of what would be the two intervening epochs in western Europe, namely: the Solutrean and Magdalenian, are lacking. The industry from Zarzi does not seem to have anything in common with the Capsian industry of northern Africa.

The expedition excavated in part a great cave at Hazar Merd, known locally as *Arshkot-i-Tarik* or "Dark Cave." This they found to contain a typical Mousterian industry belonging to the epoch immediately preceding the Aurignacian. Above the Mousterian level there were a few scattered imple-

ments of the Zarzi, *i.e.*, Aurignacian, type and at the top of the section an abundance of pottery was found. It would seem, therefore, that both the Mousterian and the Aurignacian cultures form a zone stretching accross both Europe and Asia. Whether the prehistoric current which carried these cultures moved from west to east, or the reverse, is a problem which may be solved through further exploration.

THE SPECIAL CONTRIBUTION OF DEVELOPMENTAL MECHANICS TO THE THOUGHT AND PURPOSE OF THE MAN OF TOMORROW

By OSCAR RIDDLE

(*Read April 18, 1929.*)

Any claim that some particular branch of science will particularly assist the coming generation of men to redefine the ultimate aim or purpose of human life should in meekness be prefaced by a bow to astronomy—a body of facts that has left the thought of no generation of man untouched. Let us here thus preface our claim, perhaps not meekly but with unmeasured gratitude; for, not only in the past but even today all bold and earnest thought is striving hard to "tune in" on the superb facts and ventures of the new astronomy and the new physics. *Tomorrow*, however, is another day, and it is with tomorrow that this statement is most concerned.

It will perhaps be granted that the growing thought and purpose of man tomorrow will be a development or extension of the best thought of today. We may assume also, and for the purposes of this discussion we must assume, that this best thought is now limited to leadership and that it has undergone much transformation during recent decades of a prodigious century of science. As is well known, many scientists, many dramatists, many of the ablest writers, many artists, and many others, during these decades have had their chief thought and purpose redirected from the past to something connected either with the present or with the future. For this array of intellectual leaders the soul and the angels have ceased to be things of concern or objects of thought. The synthesis or formulation of a new purpose of life—one adequately taking the place of a formula that was effective for so many centuries—seems however, not yet to be completed, or at least not to have gained a wide acceptance. In

this recent transformation of thought it is plain that astronomy, physics, biology, and psychology have each played an important part. But which kind of scientific fact and technique is destined to have most influence in supplying and popularizing a better or more satisfactory reason for the human being to accept life and to live it with assurance, with zest, and with high and unfailing effectiveness?

In the present consideration of an answer to this question we invite your consideration to the claim of one of the youngest members of the biological sciences—one whose age is less than one-fourth that of this Society.

What then is developmental mechanics—the science of which we expect so much? Some of you well know that this is the name given to a body of knowledge recently derived by applying the method of experiment to the problems of development. From many possible illustrations of the sort of facts with which it deals, and for the orientation of those who are not biologists, we may cite one or two examples. An egg is cut in two; each fragment is fertilized, *two* embryos instead of one are made to form from the single egg. Or, the two cells—even the sixteen first formed cells—in a normally developing embryo are separated by the experimenter and two, or sixteen, embryos made to form from material which if undisturbed would have formed but one. Or again, a piece of skin may be taken from the breast or back of a more advanced embryo and used to replace skin removed from the region of the developing eye; then this breast skin will be transformed into a crystalline lens and become a good and effective organ of vision. Such was the developmental mechanics of yesterday. That of today and tomorrow, we may briefly point out, is a direct and frontal attack on the control of structure, size, proportions, characteristics and capacities of organisms and parts of organisms—man included.

Much of the present and potential strength of developmental mechanics, and of its frontal attack on these problems, lies in the fact that its attack is now converging with large bodies of facts developed within the fields of endocrinology

and genetics. But there is good reason to fear that the nature and extent of this convergence will be neither understood nor granted unless this point is developed at some length. Let us therefore now have a glance at a few recent additions to the home equipment of developmental mechanics—additions which have entered either through the door of endocrinology or the door of genetics. Endocrinology first.

We may start with the enviable versatility of a salamander, the Mexican Axolotl. In its native locality this animal lives in the water, breaths by means of gills, and throughout its entire life has the other characteristics of a water animal. Generation after generation it lays its eggs and reproduces in this condition. If, however, it is transplanted to certain other regions, or if it is forced during three or four months to spend a part of the time in air, or if it is fed a little thyroid tissue, it becomes a land animal. It loses its gills, develops lungs, and becomes an altogether different individual. We may at will thus take a mature salamander and have it remain a water animal, or convert it into a land animal.

A second exhibit will show that large stature, or gigantism, can be induced in animals. An example of this sort of thing is very close at hand. When the same salamander just described is permitted to feed on a specific substance—the anterior lobe of the pituitary gland—this salamander becomes approximately twice as large as when it feeds on its normal diet of earthworms. The same result in the rat, obtained in a similar manner, has been reported by other investigators. In these cases very large stature is a gift within the power of the experimentalist who is learning the use and special power of a specific hormone even while this substance is obtainable only in a very crude form. But this internal secretion is of much significance to this discussion, since active and able attempts are now being made to isolate it in pure form; and because, when that is accomplished, this product will unquestionably and immediately be used to increase the growth of human beings. The medical profession will doubtless use it first of all to bless the diminutive among us; but there is much

reason to expect that with this agent the physician, or perhaps the experimentalist, will also be able to add a cubit or two to the stature of the offspring of all of us.

Another and last example of endocrine control. The eggs of birds contain a female or feminine hormone. They all get it because they develop in the body of a female, and because the hormone is soluble in the egg substance. But some of these eggs must develop into males and others into females. Are there consequences of this clearly accidental and adventitious dosage of the growing male embryo with a female hormone? Observation and measurements suggest that very distinct effects are thus produced on the germ glands of the male embryos. The testes of the male embryos at hatching are only about half as large as the ovaries of female embryos, despite the fact that the gland of the mature male is three or four times as large as that of the mature female. To the year 1929 we had supposed that it was "normal" for the testes of these embryos to be thus diminutive merely because it was *usual* or invariable. This case therefore also sharply illustrates the fact that in development the *usual* may be grossly repressive and abnormal; or stated otherwise, when we use the word "normal" in development we can mean only the "usual", without any decision whatever as to whether the "normal" is more or less advantageous than derivable "abnormals."

Perhaps these three exhibits sufficiently indicate that endocrinology is contributing some sharp tools and well-done furnishings to the growing edifice of developmental mechanics. Many additional examples could be cited. Instead, let us examine two gifts of genetics to the science whose aim is the control of developmental capacity in organisms.

In the first case a genetic study of a certain stock or race of the fruit fly, *Drosophila*, showed it to be provided with hereditary factors which usually resulted in the formation of extra numbers of legs. When a bit of the method of developmental mechanics was applied to the further study of this race it was found that if the eggs and young are merely grown

at *high* temperatures, instead of at the usual lower temperatures, they do *not* form the extra legs. They then masquerade as quite normal individuals.

As a last example we may cite the control of a characteristic no less widespread among organisms, and no less deep-seated within an organism, than sex. In several lowly organisms, and at least in such higher ones as the frog and bird, the sexuality of many individuals has been changed or reversed by the experimenter. In the case of the frog so simple a matter as forcing the eggs to undergo an early development in an abnormally high or an abnormally low temperature probably changes the sex of some individuals. Other methods of changing the sex of frogs, birds and other animals are perhaps a bit more complicated and need not enter into this discussion. Our point here is that a remarkable characteristic, over which the chromosomes or genetic factors usually and normally exercise a rigorous guidance and control, can be forced under certain specific conditions to develop into an alternative or opposite characteristic.

These few examples, each revealing a special side or facet of the plastic nature of development, must also serve to show a marked and fruitful convergence of endocrinology, and one aspect of genetics, with developmental mechanics. This combination or merger is all the more remarkable because it involves three of the youngest branches of biological science: Developmental mechanics dating from 1883; endocrinology practically from the nineties; genetics from 1900. Thus the age of each is a scant generation. May not this youthful union speak with some confidence of tomorrow?

Of the recent developments which lie purely within the original territory of developmental mechanics we have not spoken and shall scarcely speak here. But what might not be said of such triumphs in lower organisms as the control or reversal of polarity? Of the many revelations concerning symmetry and asymmetry? Of the cells of the embryonic heart of the chick still alive and undergoing cell-division after 17 years *in vitro*? Of two eyes or one in fish from an-

aesthetized eggs? Of induction and organizing centers in early development? To the initiated each of these things recalls whole fields of exploration and discovery. Let us here merely draw the conclusion that the few examples of developmental control that have been noted, and those fields of discovery which were merely named, all supply clear indications that during *early* life-stages *every* marked change in the environment is accompanied by a marked change in the ensuing development; and that they display *mechanism* in astounding degree and perfection. There are those who have found ways of lightly sliding over the fact of organic evolution without becoming at all awakened to what now goes on in the living world. The expansive facts and principles of developmental mechanics, once they are assimilated by this group of individuals, would seem capable of provoking some violent and productive thought. This should result in a better agreement in the thought of the intellectual leaders of today.

In the following interesting way the plasticity to be found in development was recently expressed by Sir William Hardy: "The more there is known about living matter the more there is revealed a curious simplicity. Speeman finds skin transmuted to brain or brain to skin, but the agent which effects the change appears to be a chemical substance probably of quite ordinary character. You may lead living matter as you may a donkey with a carrot—but you have to choose the carrot with some care."

We can effectively see the strong position of developmental mechanics, in its capacity to influence the thought and purpose of man, only when its relation to heredity is made clear. In considering this point we must unfortunately deal for a moment with a definition—indeed we must proceed to a rather unorthodox *re*definition. Most of genetics is concerned, first of all, with the rules or laws governing the distribution of parental genes, or groups of genes, in offspring and of obtaining visual, and sub-visual, knowledge of the localization of these genes and groups of genes. This is a

magnificent task in process of amazing accomplishment; and it is a task which genetics shares with no other branch of science. But genetics is also concerning itself with the effects on development of the presence or absence of one or more genes, and it is this effort of genetics that is converging with, and notably supplementing, developmental mechanics. We have already noted that an important body of such convergent facts has already been developed, and this part of genetics we here consider a part of the developmental mechanics of today and tomorrow.

Now to the question of redefinition. On all sides we may still hear the words—heredity *versus* environment—as though these words involve a contrast that is real. Our present knowledge of the processes of development has accomplished a redefinition of the word heredity. So far as we know, or have reason to suspect, there are two and only two general types of influences which decide the nature and course of development—and the production of finished characteristics—in organisms. One type of influence is exercised by the genetic factors, or genes. That these influences will be essentially similar in offspring and in parent is assured by the mechanism of cell-division which gives rise to egg or sperm. The other type of influence on development is exerted by the environment; or, since the word environment is usually used to express only a late and lame fragment of the actual environment, I prefer to speak of these agents collectively as "specific conditions." That these influences—particularly the very early and doubtless more significant ones—will be essentially similar in offspring as in parent is assured by the unfailing fact that egg and sperm are formed in and from the parental body.[1] Thus both the several types of molecules (genes, and myriads of non-genes) and the special adjustments of the cell forces (the more significant environment) prevailing in this little universe of solutes and of molecules and colloid surfaces, are assuredly those derived from the parent. The terms 'heredity' and

[1] Further, a fairly similar environment for later developmental stages is usually obtained by the circumstance that egg and sperm usually unite and undergo development in nearly the same place or region as did their parents.

'environment' both require a reexamination which falls little short of redefinition.

There is no development whatever, no heredity whatever, which is not the product of interaction of genes with an array of specific conditions. And these specific elements of the environment are not things exclusively hedged off somewhere outside the cell or organism; they unquestionably exist in considerable diversity at the surfaces of atoms, molecules, genes, and protoplasmic units of every order. We can properly contrast the rôle of genetic factors with the róle of specific conditions—i.e., of internal and external environment. But the interaction of these two things is necessary to the simplest step or trace of development. Only those things are said to be "inherited" which develop and have an existence. In this inheritance, gene and specific condition (environment, internal and external) are equally represented, since nothing whatever results in the absence of either. Jennings has said it well: "Possibly we should be better off with no such concept as heredity; then analysis would be correctly directed toward understanding, in organisms as in other things, in what ways there is dependence on the stuff they are made of; in what ways on the conditions in which that stuff is found."

Heredity considered as resemblance to ancestry, and *variation* as difference or divergence from it, are concepts now only partially tenable. It is often as impossible, *a priori*, to exclude environment (i.e., specific conditions) as the causative agent of a *resemblance* as it is to exclude environment in accounting for a *difference*. Resemblance and difference alike are traceable sometimes to specific conditions and sometimes to genetic factors. Possibly a greater share of the resemblance among organisms is traceable to genetic factors than to specific conditions; but it is a great mistake to assume, for example, that the difference between the egg or sperm of the sea urchin and that of the human consists merely in the difference between their genetic factors. Whatever the molecular structure of each of a thousand genes of a sperm of either species, that structure is neither attained nor

reproduced (in each cell division) except under quite *specific conditions*—at least not unless these particular molecules form an isolated exception to all the molecules known to chemist and biochemist. This elemental and neglected fact is destined to recast much conventional thought concerning heredity. It is much more reasonable to assume that the egg or sperm of the sea urchin differs greatly from that of man not only in its genes but in the intimate specific conditions which are associated with its entire composition and structure.

In the related process of organic evolution we may also advantageously regard a truly creative advance as the advent of a new molecular arrangement (a gene) *and* a new specific condition—*both capable of perpetuation in growth and division.* It may be fruitless to debate which of the two came first and is of first importance; or even fruitless in some of these now impenetrable cases to attempt an ultimate distinction between that which is molecular arrangement and that which is intimate specific condition; but, some comparable distinctions are possible in the realm of synthetic organic chemistry, and here it is unquestionable that the attainment of a particular molecular architecture depends upon both specific substance and specific condition. It may also be pointed out that the known facts assign no special value to either member of the pair—gene or specific condition—necessary to development. The one seems as blind and adventitious as the other; it is only the union of the two that has significance to life.

The current use of the phrase "heredity *versus* environment" is therefore now found to mean little—except confusion. It either denies or utterly fails to recognize the above-described conception of development. Developmental mechanics, genetics and endocrinology all provide facts compatible only with this conception. But we are now prepared to see how this conception of development (heredity) gives to developmental mechanics greatly reinforced prospects for usefulness. In the first place it becomes evident that what we have called "normal" in development is merely the *usual* in development. This is to say that a given combination of genes subjected to a certain

array of specific conditions has yielded a particular type; but that type could have been changed by changing either the combination of genes or the combination (or seriation, or intensity, or value) of the specific conditions. We here arrive at a major point in this discussion. We know no practical means of assorting at will the combinations of genes which are to enter into a human being, or into the whole human race of tomorrow; but, the road to developmental changes in human beings—through changes and controls of one or another of the multitude of specific conditions—is nevertheless open, and is daily being more widely opened. Further, since the so-called "normal" is merely the usual—under the given conditions and a particular genetic combination—we should thus be able to provide the man of tomorrow not merely with abnormal but with supernormal and beneficient developments.

To produce these changes in an individual and in a race it will of course be necessary to bring about changes in specific conditions during various stages of life and development. Some of these special conditions will doubtless have to be applied during intrauterine—or even in preuterine—life, some in childhood, some in adolescence and some in later life. This will mean indeed a sort of super-medicine, a super-education, and a continuous and very personal application of a great body of knowledge. It will be a recurrent work, necessary in every generation, as is today all the work of education and of medicine. The development of a generation of giants for example would mean no transmission or "inheritance" of gigantism, since no change is made in the combination of genes carried by these giants. If each next generation wants its giants it must make them, just as it must now make its educated men. But here we get a closer view of how this powerful creative work on the part of man may affect the thought and purpose of the man of tomorrow. If on the streets and crossroads this man may meet the man-made giant, or the man rendered specially resistant to most forms of disease, or a man with overtowering intellect—all specially perfected by the knowledge, skill and effort of man himself—will he not take

cheer in the greatness and power of man? And with such added capacities what limits are set to the things which the superior men of that generation may do to complete man's conquest of nature, to transform his method of living, and to provide a new and mellow tint to every outlook upon life? Too, since tomorrow is not yet here, we may not forget that the great masses of men today still look variously backward. Which other science will reach and equally impress the mind of the man on the street—on the Ganges or the Hoang-Ho—and have so large a share in turning the faces of the multitude to the future?

Developmental mechanics—recruited from parts of endocrinology and genetics, and supplied with ever new and incisive munitions from biochemistry, biophysics, immunology, and their kindred—already conquers and bends some of the major processes of living and of becoming (development). Its province is the field of living matter where, naturally, every new grouping of atoms brings with it new properties, and where every new molecular property provides a changed environment leading to or involved in the next step in advance. It is concerned with the mechanics of the building of body and mind; and, having served a glamourous apprenticeship in the animal world, it is rapidly preparing to cope with some aspects of human development. Its revelations concerning the living stuff—rather technical and abstruse—are destined to color the thought of leadership; its prospective applications to human structure and capacity—very clear and obtrusive—seem destined to catch the imagination and recast the thought and purpose of coming generations of man.

SOME PROBLEMS OF RAILROAD CONSOLIDATION

By EMORY R. JOHNSON

(*Read April 18, 1929*)

The problems that have arisen in connection with efforts to bring about a consolidation of American railroads into a limited number of large systems of approximately equal strength are of present importance. This fact is evidenced by several questions now receiving public attention.

During the last session of Congress the Senate Committee on Interstate Commerce of which Professor Simeon D. Fess was chairman, have prepared a bill to amend the Transportation Act of 1920 so as to facilitate railroad consolidations. A report was prepared and submitted with the bill with the request on the part of the Senate Committee that Congress and the public give consideration to the problems of railroad consolidation during the present calendar year with the hope that Congress may legislate upon the subject at its next regular session.

For several years the eastern trunk lines have been endeavoring to agree upon a plan of grouping of railroads north of the Ohio and Potomac rivers into four large systems, the divergent interest of the carriers having thus far prevented them from uniting upon a plan to submit to the Interstate Commerce Commission. In view of this situation the Baltimore and Ohio Railroad and the Chesapeake and Ohio Railroad have each submitted a plan for the enlargement of their systems, these consolidations to be two of the proposed four to include the railroads in the trunk line territory between the Mississippi River and the North Atlantic Seaboard. These petitions are now pending before the Interstate Commerce Commission which will probably not act immediately but will wait for a while with the expectation that the New York Central and the Pennsylvania Systems, which will naturally be the other two enlarged systems, will submit proposals to the Commission for

consideration along with the petitions of the Baltimore and Ohio and the Chesapeake and Ohio.

Business interest, both individuals and organizations, have much at stake in the proposed consolidations. There are naturally differences of opinion as to details of the grouping of railroads in the process of consolidation, and opposition has already developed to the proposed allocation of particular lines to some systems. This opposition is well illustrated by the action taken by the business organizations of Philadelphia which have united in requesting that they be heard by the Interstate Commerce Commission in opposition to the inclusion of the Philadelphia and Reading Railway in the enlarged Baltimore and Ohio Railroad system. The Philadelphia and Reading, which controls the Central Railroad of New Jersey, is regarded by Philadelphia as the line that is especially devoted to the industrial and commercial interests of the port of Philadelphia. It is feared by the business men of the city that a railroad that is especially interested in the development of Baltimore and is at the same time concerned with the competition of other railroad systems at the great port of New York will give less attention to Philadelphia commerce than is now being given by the Reading Railway. In a similar but less intensive manner some of the business men in Baltimore question the advisability of allowing the Baltimore and Ohio to absorb the Western Maryland. The Western Maryland's relation to Baltimore is much less important than that of the Reading to Philadelphia, but it is natural that there should be a feeling among business men in Baltimore, at least among some of them, that the Western Maryland, although one of the minor railway systems of the country, should be maintained as an independent line with the hope that it will, in time, grow stronger and provide Baltimore with three instead of two vigorous trunk lines.[1]

These illustrations are sufficient to indicate that railroad

[1] Since this address was delivered the Wabash Railway Company has announced that it will file an application with the Commission asking approval of a proposed fifth system. The proposed Wabash System would reach Baltimore via the Western Maryland Railroad.

consolidation raises public questions of importance to which careful thought should be given with the hope of reaching a wise solution.

How the Present Problems Arose

Prior to the World War public policy as expressed in law was opposed to railroad consolidation. For 26 months the government, as a war measure, operated the railroads, and in order to secure maximum service at minimum cost the government ran the railroads as a unit. This action by the government indicated to the public and to Congress that railroad integration and consolidation ought to be favored when the lines were returned to their owners. Accordingly, the Transportation Act of 1920 inaugurated a policy of government regulation and guidance of consolidation. There was a complete change from public opposition to government promotion of railroad consolidation. In adopting the consolidation provisions contained in the Transportation Act of 1920, Congress sought to bring about, through the agency of the Interstate Commerce Commission, a grouping of American railroads into a limited number of systems of relatively equal strength, the systems to be so worked out in grouping as to maintain competition and so far as possible the existing channels of commerce. The Interstate Commerce Commision was directed to prepare and promulgate a plan of grouping to which the several permanent systems were to conform.

The consolidation provisions of the Act of 1920 have not worked out in practice as was expected. The Interstate Commerce Commission prepared a tentative plan for the grouping of railroads, but the plan has never been promulgated. The Commission realizes that any plan that it might adopt in advance would probably have to be modified in the actual grouping of railroads. It was also manifest that the definite allocation of lines to particular groups in advance of the acquisition of those lines by the corporation working out the consolidation might increase the expense of acquiring lines and might delay their acquisition. On three occasions the

Interstate Commerce Commission has requested Congress to be relieved of the duty of promulgating a plan of grouping railroads, but Congress has thus far not relieved the Commission of the duty.

The result has been that nothing has been accomplished in effecting complete mergers of railroads as contemplated by the Act of 1920. The law permitted one railroad to acquire the control of another railroad by the purchase of stock or by lease, provided the Interstate Commerce Commission approved of the transaction and provided the consolidation did not bring the railroads "into a single system for ownership and operation." Since 1920 numerous railroad consolidations have taken place under this provision (Section 5, Paragraphs 2 and 3 of the Interstate Commerce Act) but no complete mergers have been effected under the other provisions of the Act (Section 5, Paragraphs 4 to 6).

Problems that Have Arisen in Connection with Railroad Consolidation

In endeavoring to carry out the policy of the Transportation Act of 1920 which, in Section 5, provided for government regulation and guidance of railroad consolidation, several specific problems have arisen which must be understood and solved if the railroads in the United States are ever to be brought together into a limited munber of systems of relatively equal strength, as was contemplated by Congress and as is generally admitted to be desirable.

1. In the first place the law has been ineffective in bringing about the complete merger of two or more railroad systems into one system owned and operated by a company taking the place of those merged. Whether or not this is the plan that would naturally be followed in voluntary railroad consolidation is somewhat uncertain, but in any event mergers have been prevented by the fact that none could take place until the Interstate Commerce Commission promulgated a general plan for the grouping of railroads and such plan has not been adopted and published. It was primarily to bring about the

complete merger of railroads into a limited number of systems rather than their consolidation by lease and stock purchase that Congress adopted the consolidation provisions of the Act of 1920. The main purpose that Congress had in mind in this respect has not been realized.

2. The Interstate Commerce Commission, in passing upon petitions for approval of consolidations as submitted to the Commission during the first few years that the Act of 1920 was enforced, was disposed to permit consolidations by lease or stock purchase if there seemed to be no definite or valid objection to the proposal. Later, however, the Commission changed its policy by requiring a carrier petitioning for the approval of a consolidation to show that the public would be benefited thereby. It was no longer sufficient for the petitioner to show that the public would not be injured, the burden of proof was upon the applicant to prove that the public would gain from the consolidation. This has inevitably increased the difficulty of bringing about actual consolidations by lease or stock purchase, when there has been any considerable opposition on the part of interested sections of the public.

3. The Interstate Commerce Commission, as time has gone on, has given increasing consideration and weight to the objections of minority stock holders who have protested against proposed consolidations. The motive of such protestants may conceivably be disinterested, but may not always be unselfish or commendable. Minority stock holders may object to consolidation not only to protect the property of the company as a whole, but to secure a maximum price for their individual shares. Minority stock holders have successfully blocked some of the larger proposed consolidations, and it is evident that the law must be so amended as to make it possible for consolidations that are in the interest of the general public to be brought about in spite of the opposition that may be raised by minority stock holders.

4. The Interstate Commerce Commission during the last few years has more and more definitely adopted the policy of

requiring the large railroads that ask approval of consolidations to include in the proposed enlarged systems such short line railroads as may desire to be taken over and incorporated in the new grouping. The financial success of most short lines depends upon the interchange of traffic with the strong roads with which they connect. If these short lines are left out of consolidations they may be forced out of existence thus depriving their owners of their property and limiting the transportation services now enjoyed by the public. Not all short lines desire to be included in grouping as proposed, and it does not follow that the Commission will hold it to be in the public interest that every short line that desires to be included in a consolidation should be made a part of the enlarged system. The Commission now has a definite policy in regard to short lines which may be stated as follows:

When a big company like the New York Central proposes to incorporate within its general system additional main line railroads, it must also provide for including in the consolidation such short lines as may desire to be acquired and as, in the judgment of the Commission, should, in the public interest, be included in the proposed consolidation. There is now pending before the Commission the petition of the New York Central Railroad to lease the Michigan Central and the Big Four systems which are now controlled by the New York Central through stock ownership. There are several short lines connected with New York Central and with the other roads that are to be brought together into closer unity. The Interstate Commerce Commission has given its general approval of the proposed consolidation, but has stipulated that the New York Central shall, within six months, endeavor to acquire by negotiation certain designated short line railroads. Other enumerated short line railroads need not be included. If the New York Central is not able to purchase a short line railroad designated by the Commission to be included, the New York Central is to report to the Commission that it has been unable to agree with the short line as to a reasonable price. The Commission will then fix a price

for which the New York Central may acquire the short line. The price paid for all lines acquired must be approved by the Interstate Commerce Commission. The railroads have accepted this policy on the part of the Commission, and this seems to solve what is known as the short line problem.

5. The financial plans followed in the consolidation of railroads have given rise to certain other problems that may not be easy to solve. One plan for financing consolidations has been for the individuals who are to be the principal owners of the consolidated properties to form a securities or holding company with a limited amount of stock that is subject to close personal control. This securities company may then purchase the stock of the several railroad companies to be consolidated or, as was the case in the proposed Nickle Plate consolidation, the securities company may purchase control of a railroad company that has acquired the stock of railroad companies to be brought together into a consolidation. It is interesting to note that although the Interstate Commerce Commission was of the opinion that the original Nickle Plate consolidation attempted in 1925 would be advantageous from a traffic point of view, the Commission's approval was withheld because of the proposed financial arrangements. It was the judgment of the Commission that there was too great a concentration of control in a securities company.

The holding company by which a consolidation is brought about may not be a carrier subject to the Interstate Commerce Commission, although the stock of the holding company may be owned by a railroad company. The Pennsylvania Railroad is commonly reported to have bought a large share of the stock of the Lehigh Valley and Wabash railroads. What happened was that the Pennsylvania Company bought something over 30 per cent. of the stocks of the Lehigh Valley Railroad and a larger percentage of the stock of the Wabash. The stock of the Pennsylvania Company is owned by the Pennsylvania Railroad Company. Whether the purchase of a controlling interest in the Lehigh Valley Railroad and of the Wabash Railroad by the Pennsylvania Company would be

subject to the approval or disapproval of the Interstate Commerce Commission is a question that has not been decided. The courts may have to pass upon the question. Further legislation may be necessary in order to prevent the evasion of the provisions of the Transportation Act of 1920 as regards the government regulation and guidance of railroad consolidation.

Solution Proposed by the Fess Bill

As has been stated, the provisions of the Interstate Commerce Act regarding railroad consolidation as contained in Section 5 Paragraphs 2 and 3 permit one carrier to acquire other carriers "either under a lease or by the purchase of stock or in any other manner not involving the consolidation of such carriers into a single system for ownership and operation" provided the Interstate Commerce Commission is of opinion that such acquisition will be in the public interest. This Section would be amended and made more effective by the bill introduced into the Senate at the last session of Congress by Senator Simeon D. Fess, Chairman of the Committee on Interstate Commerce. The proposed amendment makes it incubent upon the Commission not only to decide whether a proposed consolidation by lease or stock purchase will be in the public interest but also to issue a definite finding and order, if the Commission's opinion is favorable; and after making such finding and order the Commission may subsequently from time to time approve and authorize the consolidating company to acquire carriers under such rules and regulations and under such terms as may be prescribed. The significance of this amendent is that a carrier proposing to acquire other railroads may set forth its proposed consolidation plans to the Commission and find out whether the Commission considers such a consolidation to be in the public interest. If the Commission favors the consolidation then the terms and conditions to be observed in carrying out the consolidation can be determined. A carrier does not have to work out its consolidation plans in detail before ascertaining whether the proposed grouping of roads is considered by the Commission to be desirable.

Paragraphs 4 to 6 of Section 5 of the Interstate Commerce Act were intended to bring about the complete merging of railroad corporations into a limited number of corporations to take the place of those merged. This plan has failed to work for reasons already set forth. The Fess Bill would repeal Paragraphs 4 to 6 of Section 5 and substitute a different plan for consolidations and mergers. Under the proposed plan the Commission will not have to promulgate in advance a general plan for the grouping of all railroads, a plan to which individual mergers must subsequently conform. The Commission is to pass upon each individual consolidation or merger upon its merit.

Minority stock holders under the proposed bill cannot prevent the majority from effecting a lease or sale of their property to another carrier. Provision is made for the appraisal and condemnation of the stocks of the minority stockholders and the acquisition of such stock by the company that is effecting the consolidation.

The Fess Bill also protects the interest of short line railroads in accordance with the plan that has already been adopted in practice by the Interstate Commerce Commission. The Commission may require a company that petitions for the approval of a consolidation to make additional carriers party to the consolidation. An effort is to be made by the consolidating carrier to acquire the additional lines specified by the Commission, and "if, after hearing, the commission is of the opinion that the carrier that is to be made a party is insisting on unreasonable terms, the commission may revoke or modify the condition or, if requested to do so by such carrier, may prescribe the terms on which the carrier may be made a party to the proposed unification."

It is sometimes more difficult to secure legislation upon a subject of general interest to the entire public than it is to bring about the enactment of a law of particular concern to a special interest. In order to get Congress to focus its attention upon the subject of railroad consolidation it is necessary for the general public as well as the carriers to concern them-

selves with the problems to be solved and with the measures that should be taken to bring about a wise solution. It is to be hoped that the press will emphasize the discussion of railroad consolidation, particularly during the months preceding the opening of the next regular session of Congress, and that wise constructive legislation may be enacted within the next twelve months.

THE NUTRITIONAL PROPERTIES OF MALIGNANT CELLS

By ALEXIS CARREL

(*Read April 19, 1929*)

The progress in the knowledge of malignant cells, made during the last two years, has been brought about by the use of a new method. Through this method, the specific elements of tumors can be directly apprehended and studied while living in an independent manner outside of the organism. Neoplastic cells, so far, had been observed exclusively in a dead condition, in sections of tumors which were fixed and stained. Their morphological appearance was ascertained, but their physiological properties could not be investigated. Their functions were assumed to be identical with those of the tumor. The "malignant cell," as described in classical treatises, is merely a mental construct.

A few years ago, a method was developed in my laboratories for the purpose of discovering the elementary characteristics of normal tissues. It consists of the analysis of the physiological properties of the various cell types, after they have been isolated in a pure state and maintained *in vitro* under definite nutritional conditions. Tissues and blood cells, instead of being known only by their appearance, as previously, are defined in a far more complete manner by their morphology when in a medium of known composition, their mode of locomotion as recorded by cinematography, the architecture of their colonies, their residual activity, their rate of growth, the manner in which they modify their medium, their secretions, their food requirements, etc. In this way, it has become possible not only to obtain a far more profound knowledge of normal tissues, but also to predict in some measure their behavior within the organism.

Such is the method that has been applied to the study of neoplasms. For this investigation, animal instead of human

tumors have been used, because the malignancy of the cells has to be tested by inoculation, and that obviously cannot be done on human beings. We have succeeded in obtaining pure strains of the macrophages of Rous and other fowl sarcomata, and of the fibroblasts of Jensen sarcoma and of Sarcoma 10 of the Crocker Foundation. Fischer has made an extensive study of the epithelium of Ehrlich carcinoma. The specific element of the Flexner-Jobling carcinoma has also been isolated by Laser in Berlin. In addition, I have studied nine spontaneous cancers of the mouse. When these neoplastic cells were compared with normal macrophages, fibroblasts, and epithelial cells living also in pure cultures, the properties which characterize each type became evident.

It appeared at first that sarcomatous and carcinomatous cells do not possess any morphological characteristic which is specific. The fibroblasts of Jensen sarcoma, for instance, keep their malignancy after several months of life *in vitro*. However, the morphological abnormalities which they show when they are part of a tumor disappear after they have spent a few weeks outside of the body. Such abnormalities should be considered only as secondary manifestations of malignancy. There must be, however, some visible or invisible change in the nuclear or cytoplasmic organs, because a new function cannot be conceived without a structural substratum. This structural change has not as yet been discovered. Malignant cells often are diseased and show evidences of degeneration, but this characteristic is not essential. Thus we have observed *in vitro* for long periods of time colonies which were perfectly sound and grew indefinitely. Their component cells never died. It is certain that the pathological condition displayed by the neoplastic elements is not essential. Malignancy may coexist with a healthy state of the cells.

Such cells, contrary to the classical conception, do not possess a greater rate of growth than normal cells of the same type. Their residual energy is also no higher. Their mode of locomotion and the architecture of their colonies do not

differ markedly from the normal. For instance, sarcomatous fibroblasts form in the culture medium a tissue with a texture that closely resembles that of a normal strain.

Neoplastic cells are not anarchical. They are as law abiding as normal tissue elements. But they differ from the normal in certain properties. Their colonies set free more acid than normal tissues, as previously observed by Warburg. They also liquefy the plasma forming the coagulum in which they are cultivated. Although liquefaction is not a specific characteristic of malignancy, it generally accompanies the growth of the tumor elements. Malignant and normal cells differ chiefly in their food requirements. For instance, normal fibroblasts, as is well known, do not multiply indefinitely in a medium composed of blood serum. In other words, they do not obtain from the serum proteins the nitrogen necessary for the synthesis of protoplasm. On the contrary, the fibroblasts of Jensen sarcoma, the epithelial cells of Fischer carcinoma, and those of the mouse tumors which I have studied, multiply actively in rat serum. Fischer epithelium differs from other carcinomata because it also feeds on fowl serum and on embryo tissues. The epithelial cells from mouse cancers generally die when cultivated in fowl serum. It must be noted that these tumors also differ from Ehrlich carcinoma because most of them cannot be propagated by transplantation. In some measure, each tumor strain possesses its nutritional peculiarities. Neoplastic epithelium has acquired the nutritive properties of normal macrophages. Like those cells, it feeds on blood serum. However, the fibroblasts of Sarcoma 10, which is very malignant, do not feed on serum proteins. They utilize embryonic juice, as normal fibroblasts do. But they differ from the normal because they multiply much more actively when in contact with the ameboid cells of bone marrow. They also grow indefinitely in the proteoses and peptones of hydrolyzed liver. They even have the property of feeding upon the amino acids resulting from a more complete digestion of the liver, as long as some peptides are present. On the contrary, normal cells cannot

obtain from these substances the nitrogen necessary for the synthesis of protoplasm.

Malignancy seems, therefore, to consist in a change in the mode of alimentation of the cells. While these cells do not possess an inherent growth energy greater than the normal, they are able to procure the nitrogen required for their multiplication from substances which are within their reach in the organism. A normal cell can be compared with a motor which runs as fuel is supplied. When a cell is immersed in a fluid containing substances which it can use in the construction of protoplasm, it cannot help multiplying. Since the classical conception of the malignant cell has almost completely collapsed, another should be substituted, based on the nutritional properties of each cell type. In the last analysis, malignancy is due to an increased production of proteolytic ferments and of acid, and to the resulting aptitude of the cells to feed upon substances present in the interstitial fluids.

A SKETCH OF THE LIFE OF JOHN BRADBURY, INCLUDING HIS UNPUBLISHED CORRESPONDENCE WITH THOMAS JEFFERSON

By RODNEY H. TRUE

(*Read April 19, 1929*)

AMONG those early nineteenth century explorers who in the days following the purchase of Louisiana and the return of the Lewis and Clark Expedition sought to learn more of that far country, was the English traveler and naturalist, John Bradbury. Of this man little seems to be generally known beyond what can be gathered from his Travels, published in Liverpool in 1817. Britten and Boulger,[1] in their biographical sketches of British and Irish botanists, volunteer little concerning the man himself beyond the fact that "he flourished between 1805 and 1813."

Concerning his years in America a considerable body of information has been brought together, but until somewhat recently the date and place of his birth, as well as the story of his earlier years, have apparently been forgotten. Fortunately this information has come to light in a book of local history entitled "Bygone Stalybridge," [2] written by Samuel Hill.

[1] Britten, James and Boulger, G. S., "A Biographical Index of British and Irish Botanists," 1893, p. 21.

[2] Hill, Samuel, "Bygone Stalybridge." Stalybridge, 1907. Published by subscription. Through the interest of Mr. J. L. Wilson of San Francisco, whose father was a friend of the historian—and, Mr. Wilson says, poet also—the attention of Judge Walter B. Douglas, Vice-President of the Missouri Historical Society, was directed to Hill's account of the early life ot Bradbury, and extracts from his book were forwarded. The Missouri Historical Society in its turn, through Judge Douglas, has most generously placed the correspondence with Mr. Wilson and the chapters from Hill's book at the service of the author of this sketch, who desires to express his sincere appreciation of this courtesy on the part of the Society and especially of its Vice-President, now deceased. The writer is likewise very greatly indebted to Dr. Joseph A. Clubb, Curator of Museums at Liverpool, for a transcript of the same chapter from Hill's book sent on December 28, 1916, in reply to the inquiry concerning Bradbury's life. This service is the greater since the book itself has become so rare that it is not to be found even in the British Museum. This debt to Dr. Clubb is increased by his inquiries in the library at Stalybridge and in Liverpool, into the records of the scientific institutions there. As a result of his search, Dr. Clubb is inclined to regard Bradbury's relations with the cotton industry as more important than his connection with the Botanic Garden.

According to Hill's account, John Bradbury was born at Souracre Fold, or Far Souracre, Stalybridge, England, about the year 1765. The Bradbury family numbered seven persons all told, there being besides their parents four sons and one daughter. As a little lad John went to a school in the neighborhood taught by John Taylor, who took special interest in mathematics and botany. At an early age Bradbury developed a great interest in natural history and out-of-door life, a leaning discovered and encouraged by his teacher who sometimes took the boy with him on his botanical excursions. This interest in plants was further stimulated by the boy's father who bought him a copy of the works of Linneus, which, his biographer says, "he studied fervently."

Even as a boy, however, he was obliged to leave school and work in one of the primitive cotton mills which at that time had been put in operation in the town, but he still found some free time to devote to his studies. At the age of eighteen he organized a night school among the young men of his acquaintance and he himself became its teacher. He even bought a microscope and other apparatus which he used in his instruction. His own studies on plants and insects were prosecuted with such enthusiasm and discernment that it is not a matter of surprise that the results of his observations began to receive considerable notice. At the age of twenty-two his writings and discoveries came to the attention of the naturalists of London, notably of Sir Joseph Banks who wrote to Bradbury. As a sequel the young naturalist visited London where he met with a reception that would have been flattering to older men than he. He was introduced at the Linnaean Society and was made a member.

Bradbury is said to have given some attention about this time to landscape gardening, organizing and laying out country seats and parks for Sir John Parnell, the Duke of Leinster, Mr. Legh of Lynne, and other wealthy amateurs of botany and natural history. At this period he made a walking tour of Ireland which is said to have yielded him many new plants.

Concerning the years of Bradbury's middle life, Mr. Hill's

account tells us little, but we get a glimpse of this period from other sources. Early in the last century he seems to have lived both at Manchester and at Liverpool, between which places he vibrated. At Liverpool he met William Roscoe, with results that are to form an important part of our narrative. Here he also made the acquaintance of Mr. Bullock, then at the head of the Liverpool Museum, and became actively engaged as corresponding secretary in the work of the Liverpool Philosophical Society, an organization promoting the diffusion of scientific knowledge.

Among the patrons and supporters of this society were the Earl of Derby and Col. Leigh-Phillips, through whose influence perhaps the organization interested itself in the increasing demand for a larger supply of cotton for the manufacturers of that part of England. The supply had been dependent upon the West Indies and it appears that the possibilities in this direction existing unknown and unexplored in the vast new areas of the United States seemed to the supporters of the society worthy of investigation. At all events, in 1809 Bradbury was selected to undertake a journey of survey and exploration through the southern part of the United States.

At this time he was about 43 or 44 years of age and is described by an author as "being in the prime of manhood, swarthy, broad-shouldered, and of medium height, amiable yet stubborn in disposition, temperate in his habits, and a most excellent marksman. He was fond of music, active on his feet, and determined in his methods and opinions."

Armed with letters of introduction to President Madison, to ex-president Jefferson, and to the British Consul at Washington, Bradbury left England in the spring of 1809 and arriving in due time found himself warmly welcomed by American friends of science to whom his work had become known.

Fortunately, at this point, where Hill's account drops the thread, we are able from American sources of information to take it up. Among the Jefferson Manuscripts of the Library of Congress is a very interesting and illuminating correspon-

dence, for the most part between Bradbury and Thomas Jefferson, and in part between Jefferson and others having to do with Bradbury's affairs. Since but very little of this correspondence seems thus far to have been published, we shall be able to continue the story in a great measure in their words. Jefferson at that time had returned to Monticello and had entered on that long period of retirement in which his home became a sort of Mecca toward which the footsteps of so many travelers turned. Some came out of mere curiosity and spent days as guests at this hospitable place, others, like Bradbury, recognizing Jefferson as preeminently the patron of science and learning in America went to pay their homage and if possible to discuss their problems with the philosopher.

Hence it was almost a matter of course in April, 1809, that Mr. William Roscoe, speaking in the name of the proprietors of the Liverpool Botanic Garden, should give John Bradbury, then soon to leave for America, a letter of recommendation to Mr. Jefferson. This letter is here quoted in so far as it concerns our subject.[1]

"ALLERTON NEAR LIVERPOOL.
25 Ap. 1809.

Sir,

I presume upon your well known partiality to liberal and scientific pursuits, to introduce to your notice, Mr. Jn. Bradbury, a Fellow of the Linnean Society who has undertaken a tour thro' the province of Louisiana, for the purpose of collecting the various specimens of Natural History which it may be found to furnish. Among those who have encouraged his undertaking, in which he will be accompanied by his two sons, are the Proprietors of the Botanic Garden in Liverpool by whom he is requested to offer to your acquaintance a Copy of their regulations and a Catalogue of their collection. Should he have the honour of delivering these in person, I trust you will find him well informed in the different branches of his favourite science, and capable of informing you of the progress which he is here making in it. Any information, assistance, or advice, which you may have the goodness to afford him, will be gratefully acknowledged, as an obligation conferred both on him, and on those who patronize the undertaking. . . . "

[1] Jefferson Papers. Library of Congress, Series II, Vol. 73, No. 80.

This letter was received by Jefferson on August 6, 1809, probably from the hand of Bradbury himself who seems to have been entertained at Monticello about three weeks. It seems, however, that after the lapse of ten days Jefferson had so far gotten into his plans as to revolutionize them and ultimately to start him westward to St. Louis, instead of southward toward New Orleans as was originally intended.

This change of plan, seen in the light of subsequent events was probably a very significant decision for Bradbury. Had he gone to New Orleans, as his cotton-consuming patrons desired, he would have been virtually their agent at that source of supply. In deciding to follow the call of the wild, he became the explorer and botanist, at times unsupported and in distress, never long at rest. Be that as it may, on August 16, Jefferson wrote the following note of introduction to Meriwether Lewis, then Governor of Louisiana province:[1]

"MONTICELLO, Aug. 16, 09.

Dear Sir:

This will be handed to you (mss. torn) Mr. Bradbury, an English botanist, who proposes to take St. Louis in his botanizing tour. He came recommended to me by mr. Roscoe of Liverpool, so well known by his histories of Lorenzo of Medicie & Leo X who is president of the Botanical Society of Liverpool. mr. Bradbury comes out of their employ, & having kept him here about ten days, I have had an opportunity of knowing that besides being a botanist of the first order, he is a man of entire worth and correct conduct. as such, I recommend him to your notice, advice & patronage, while within your govermnent or its confines. perhaps you can consult no abler hand on your Western botanical observations. . . . "

It seems from the following excerpt that the plans made in Liverpool had provided that Bradbury should make New Orleans the headquarters for his botanical explorations.[2]

"This intention I made known to Mr. Jefferson, during my stay at Monticello, when he immediately pointed out the want of judgment in forming that arrangement, as the whole of the country

[1] Jefferson papers, Library of Congress, Series V, v. 16, No. 7[a]. This letter is quoted in large part by Thwaites in the biographical introduction in his reprint of the 2d edition of "Bradbury's Travels," p. 9.

[2] "Bradbury's Travels." First ed., p. V, preface.

round New Orleans is alluvial soil, and therefore ill suited to such productions as were the objects of my pursuit. In consequence of his representations, I changed my intentions, and proceeded to St. Louis, one thousand four hundred miles above New Orleans by the course of the Mississippi, where I employed myself, during the winter of 1810, in making such preparations as I deemed necessary for what might be collected during the ensuing summer."

It appears probable that in going from Monticello to St. Louis Bradbury followed the usual route of those days down the Ohio River. Moreover he seems to have visited Philadelphia on his way to the Ohio. At that place he met a young Englishman named Thomas Nuttall, in whom Dr. Benjamin S. Barton, the chief botanical authority of that time and place had taken an especial interest. Barton had practically made Nuttall over during the short period of their acquaintance, changing a journeyman printer into a promising young botanist, so that at the time of Bradbury's arrival, Nuttall was ready for greater undertakings. He joined Bradbury and together they proceeded to St. Louis where they arrived on December 31, 1809.[1] The following spring and summer, Bradbury says[2] were spent in making a number of short excursions, not reaching more than eighty or a hundred miles into the surrounding country. He was able in this time to get a good representation of the flora of the region, and dispatched a considerable shipment of plants to Liverpool, by the way of New Orleans.

The boat bearing this first installment was driven ashore not far from St. Louis and in the hope of saving some of his collections, Bradbury went to the scene of trouble. But he found that the boat had been repaired and had continued on her journey before he arrived. Upon returning to St. Louis he found a large party preparing to ascend the Missouri river, bound for the Pacific Coast by the overland route blazed by the Lewis and Clark Expedition. Bradbury and Nuttall being invited to join this unusually interesting group of ex-

[1] Harshberger, John W., "The Botanists of Philadelphia and Their Work." Philadelphia, 1899: 111 and 153.

[2] Bradbury, J., "Travels," etc. Second edition. 1819: 17.

plorers, and finding in the leaders Wilson P. Hunt, Ramsey Crooks, and Donald McKenzie, congenial men, decided to accept the suggestion and to travel with the party as far as suited their purposes. This expedition, organized by the Pacific Fur Company, became famous through the account of its adventures written by Washington Irving[1] in his book entitled "Astoria."

The Astor expedition was itself so much more pretentious in all respects and so much more dramatic in its purposes and subsequent history than the smaller one which attached itself to it, that Bradbury receives only a slight but nevertheless appreciative mention in Irving's account of the Astor venture.

Bradbury started up the Missouri River on the 13th of March 1811. The account of his travels details a wealth of observations on botany, zoology and geology as they were made day by day. To refer even casually to this most important part of his accomplishment would carry us far beyond the limits of this sketch. He continued with the Astorians up the Missouri River as far as the Arikara villages above the Grand River and about 1800 miles above St. Louis. From here, accompanied by Crooks, he pushed on two hundred miles farther up the Missouri to the fur trading station among the Mandans, not far from the present site of Bismarck, N. D.[2]

On returning from this excursion he found the Astor party preparing for their long trip over the Rockies to the Pacific. Although invited to continue with them, Bradbury was not assured of any means of making the sea voyage back to the Atlantic Coast and chose to return as he had come, thus being reasonably secure in the specimens and materials he had already collected. The noted fur trader, Manuel Lisa, was returning to St. Louis and accepted Bradbury's boat as pay for his passage, and for making stops in accordance with Mr.

[1] Irving, Washington, "Astoria or Anecdotes of an Enterprise beyond the Rocky Mountains." Author's Revised Edition. Putnam, New York. 1849. pp. 143, 172–174, 220.

[2] Thwaites, "Bradbury's Travels," Vol. V. "Early Western Travels," p. 148, footnote.

Bradbury's botanical requirements. They started July 17 and such was the force of the current and such was Mr. Lisa's haste that the expedition arrived at St. Louis on July 29th. No stops were made for Mr. Bradbury at the points he desired to collect, and his disappointment and indignation were keen at being thus defrauded by the fur trader.

At St. Louis he found mail from Liverpool that greatly disturbed him. Those supporting his expedition had decided to make no more remittances, perhaps because of the decision to devote himself to the Missouri expedition rather than to cotton prospects about New Orleans. He was heavily loaded not only with herbarium material but also with living plants for the Botanic Garden. Not wishing to lose the fruits of his labors, he secured a piece of ground in St. Louis from a Manchester man named Bridge, and set to work preparing it to receive his plants. Hardly were they safe in the ground and protected by a fence than he was attacked by a bilious fever which nearly proved fatal. This attack gradually gave place to an intermittent type of disorder which kept him laid up until late in November.

At about this time Bradbury received better news from Liverpool, Mr. Shepherd, the Curator of the Botanic Garden forwarding the remittance that had been withheld. He learned also that the collections made in the vicinity of St. Louis in 1810 had been received, over a thousand potted specimens and vast numbers of seedlings being secured. Perhaps this good news helped his hitherto slow recovery. At all events, learning that a young Philadelphian named H. W. Drinker who had befriended him during the tedious weeks of his illness in St. Louis, was planning to embark shortly for New Orleans with a boat load of lead from farther up the Mississippi, Bradbury was able to accept his invitation to go down the river with the party. Accordingly on December 5, 1811, they set out on their voyage. Their trip was not uneventful. Although his scalp had been in danger from hostile Indians many times during the year, it is doubtful whether Bradbury's life had at any time been more seriously menaced

than when on this trip. The stranded trees and partly or wholly submerged logs in the river frequently threatened disaster and the terrific experience of riding out the earthquake of New Madrid almost on the spot of greatest disturbance, makes thrilling reading in Bradbury's account for December 15th and 16th. (Travels, 2d Ed. p. 204, Thwaites' reprint.) On January 13, 1812, the expedition arrived safely at New Orleans and on the 20th of the same month Bradbury set sail in a boat bound for New York.

We now arrive at the end of Bradbury's own account of his travel. It seems probable that he not infrequently found Jefferson a convenient friend during his stay on the frontier. Evidence of this is found in a self-explanatory note written by the ex-president to William Pinkney, at that time American Minister to England.[1]

"MONTICELLO, July 15, 10.

"*Dear Sir*

"I again trouble you with letters from Mr. Bradbury to his friends in England. He is a botanist and naturalist of high qualifications and merit, and is now engaged in exploring Upper Louisiana. I feel a real interest in his pursuits, the result of which so far is communicated in some of these letters.

"TH. JEFFERSON."

"H. E. William Pinckney (sic)"

If any doubt of the cordiality of Jefferson's feelings toward Bradbury could survive after the expressions already quoted, they must be extinguished by a letter written from Monticello to Dr. Benjamin S. Barton of the University of Pennsylvania on September 11, 1811, at the time when Bradbury was lying ill at St. Louis. Since the letter adds a small but interesting contribution to the conditions under which Bradbury came to America it may be here quoted in part:

"You will have seen the name of a Mr. Bradbury among the adventurers from St. Louis up the Missouri and lately returned thence to St. Louis—he is an English Artisan from Liverpool, who being desirous to move his family to this country, and not free, by their regulations to come under that character, was employed by the Botanical Society of Liverpool to come out as their herborizer, he is

[1] Jefferson papers, Massachusetts Historical Collections, Series 7, Vol. 1, p. 143.

an estimable man and really learned in the vegetable, animal and mineral departments of science. he is indefatigable in his researches, and before that journey, had already discovered a great number of new articles, which he has communicated from time to time, to his employers: and has returned to St. Louis fraught, without doubt, with a great mass of information which will immediately pass the Atlantic, to appear first there. he was recommended to me by Mr. Roscoe and staid here three weeks, passing every day in the woods from morning to night. he found, even on this mountain, many inedited articles . . .

It was to be expected that Bradbury would not be long on shore at New York before resuming his correspondence with Mr. Jefferson. The first letter follows:[1]

"NEW YORK, 5 March 1812.

"*Sir:*

"The terms of my mission to Louisiana having expired, I arrived here a fortnight ago from St. Louis in the hope of receiving my family in the ensuing spring. I am here informed that the Government of the United States have it in contemplation to establish a Botanic Garden at the City of Washington and that no appointment is as yet made of a Person to Superintend it. If this information is correct I would willingly offer myself to those in whom the power of making the appointment is vested, but suppose I am totally unknown to them. If Sir you deem me qualified to fill the station you will render me greatly your Debtor by making this my wish known to those who have the authority in this business. I shall only add on this subject that my extensive acquaintance amongst the naturalists in Great Britain, together with what plants I have acquired in Louisiana, would enable me to assist the establishment considerably. I ascended the Missouri last summer to a little above the Mandan nation and found the soil and aspect of the country changed after passing the River Platte and consequently abounding in natural productions almost wholly different from those to the eastward of that River. The plants which I there collected and which do not appear to be described in the last edition of the *Systema natura* exceed 100 species some of which are beautiful. In zoology I think I shall add two species of *Crotalus* a *Talpa* a *Sciurus*, and an animal (with) cheek pouches as *Mus Bursorius* of Linnaeus but differing from that animal specifically yet agreeing in generic character and both so much disagreeing with the Genus *Mus* that I am of opinion they must constitute a new Genus betwixt *Mus* and *Arctomys*. I

[1] Jefferson papers, Library of Congress, Series II, Vol. 11, No. 125.

have an ardent wish to ascend the Arkansas and Red Rivers, confident that their borders would afford a rich harvest. If I can obtain the situation mentioned above perhaps an opportunity may be afforded me. whether or not there is any probability that I may I beg Sir you will have the goodness to cause me to be informed by Letter to the Postoffice Neward (sic) State of New Jersey.

"I am Sir your most obliged Servant,

"JOHN BRADBURY."

To this letter, Jefferson makes a prompt reply.[1]

The letter is worth quoting for the light it sheds on the notion which those in authority were likely to have of the character of a botanic garden.

"MONTICELLO, Mar. 21, 12.

"*Sir:*

"I duly received your letter of the 5th inst., and congratulate you on your safe return from your long peregrination. I hope it will not be long before we shall have the benefit of the information it has furnished you.

With respect to the establishment of the Botanical Garden at Washington by the General government, be assured it is an idea without the least foundation. no doubt it is desired by every friend of science; and it may be expected by such of them as have not sufficiently contemplated either the powers or the present circumstances of the government. there have been repeatedly applications by individuals, & one of them lately, for the use of some of the public grounds at Washington for the establishment of such a garden, and if the suspicion that it would be converted into a mere kitchen-garden for the supply of the town market can be removed, it is in the power of the President and would probably be within his disposition so to dispose of it, but I do not believe the Government will or can do more. The setting you right in this fact being the best service I can render you in the case, I do it as a duty & add with pleasure the assurance of my esteem and respect.

"TH. JEFFERSON."

Bradbury was now obliged to face the future. Without employment, he must either find it or return to England. Before he could seize either horn of the dilemma, the breaking out of the war of 1812 between his native country and that in which he now found himself quite effectually settled this difficulty for the time. He says "I waited for its termination,

[1] Jefferson papers, Library of Congress, Series II, Vol. 11, No. 127.

and made some arrangements which caused a necessity for my stay sometime longer." (Preface to Travels, 1st ed., p. VI, 1817.)

Probably the arrangement making necessary a longer stay refers to business connections which he made somewhere in the East. This attempt to establish his financial situation involved a manufacturing enterprize in which the depression of the times assisted by the bad business morals of a partner brought Bradbury to grief rather than to prosperity. After a silence of nearly four years, he again brings his troubles to Jefferson.[1]

"WARDSBRIDGE, NEW YORK,[2]
9 Jany. 1816.

"*Sir:*—

"In my Tour up the Missouri I was deeply impressed with the belief that the region south of that River was extremely rich in Mineralogical as well as Botanical Treasures, and returned with a decided intention to explore the Arkansas and Red Rivers with a view to collect (at least) the materials for a Nat. History of that country. This design I communicated to Docr. Smith P. L. S., now Sir John Edward. I received from him the most friendly promises of assistance in the nomenclature, etc. etc. I came to the Eastern States in order to raise the means by a sedulous attention to business or to obtain a situation in which such an undertaking should become a duty. I have tried the former alternative and am disappointed, partly by the present state of manufacturers in this country but more by the turpitude of the man with whom I am connected in business as partner.

"The consideration that the period of Vigour with us has other limits than Death now urges me to look round for the most speedy means of accomplishing my darling object. My wish has received an additional impulse on being lately informed that a person is now on his way to explore those region's sent from England with the intent that his discoveries shall be published in that country. If, impelled by these feelings, I make an improper appeal to you Sir impute it, I beg, to the true motive, *Zeal for Science*. I notice in the reports of the proceedings of Congress that a road is in contemplation from St. Louis to the northern boundary of Louisiana for the laying out of which commissioners are to be appointed. I am well acquainted

[1] Jefferson papers, Library of Congress, Series II, Vol. 11, No. 124.
[2] Now Montgomery, Orange Co., on Wall Kill about 12 miles west of Newberg on Hudson.

with a considerable portion of the country from St. Louis to the Arkansas, have received a mathematical education and have a complete knowledge of surveying. In the geological part of mineralogy & the external characters of fossils, I am not less versed than in Botany. As the Road will assuredly pass through the mineral country, if a mineralogical (account) would be desirable in addition to the survey, I might if employed furnish it. Should I be honored with an appointment in this business or farther in the interior (which I should prefer) I pledge myself that the results shall be published in the United States. I must apologize for obtruding myself on you and plead as my excuse that I am unacquainted with and unknown to those in power. If the application is improper you will of course suppress it.

"I am Sir your most obedient and obliged

"JOHN BRADBURY.

"P.S.—My discoveries in Botany have been published in England and are considered valuable."

This postscript refers to the publication of Pursh's *Flora Americae Septentrionalis* in 1814 in London. Pursh, after finishing his work on the Lewis and Clark plants, eventually went to England to see what he could add to his material from American sources. He went to Liverpool and found the Bradbury shipments not long arrived from America. Pursh says in his Flora [1] "I am also highly indebted to William Roscoe, Esq., who very obligingly communicated to me Mr. Bradbury's plants collected in Upper Louisiana. This valuable collection contains many rare and new species, having been collected in a tract of country never explored before: those which were entirely new I have described in the Supplement to the present work." [2]

Here appear described from Bradbury's material 39 new species credited to Bradbury only by including the name of

[1] Pursh, Frederick, "Flora Americae Septentrionalis," etc. London, 1814, Vol. 1, p. XVII, preface.

[2] Dr. Clubb, Curator of Museums, Liverpool, under date of Dec. 28, 1916 writes "Some years ago we received from the Liverpool Botanic Gardens a number of herbarium specimens, and I find there are some 8 or 10 specimens recorded as being collected by Bradbury, but what has become of the mass of his collections, which must have been considerable, we have no records."

In 1839, T. B. Hall in the *Naturalist* (London, Vol. 4: 397) writes "there are a great many of his (Bradbury's) dried specimens in the Herbarium, principally in Louisiana."

the collector with the descriptions. Bradbury felt himself ignored and practically defrauded by Pursh, an opinion shared by his fellow collector, Nuttall. Roscoe's part in turning the material over to Pursh seems curiously enough not to have called forth the outspoken resentment directed against Pursh. Both of them in other places questioned Pursh's botanical morals which came eventually to be seriously challenged by Baldwin, Short and others.

But to turn from the postscript, in which Bradbury does not reveal his disappointment, to Jefferson's reply to this appeal, we find the ex-president writing on Feruary 29, 1816, two letters having for their object the forwarding of Bradbury's interest. In one letter, Jefferson applies in behalf of Mr. Bradbury for a situation under the government and takes the opportunity to sketch the main features of Bradbury's life. Jefferson's letter is here quoted in full in so far as it bears on our subject.[1]

"Mr. Bradbury is an Englishman, a man of science particularly in Botany and Natural History. he was at the head of a great weaving establishment in Liverpool, which the pressure of their taxes obliged him to break up. desirous of coming to this country to seek an establishment, he got an appointment from the Linnean Society of Liverpool to botanize for them in Louisiana. He came over in 1809 or 1810, brought me a strong letter of recommendation from Mr. Roscoe, staid with me about 3 weeks and went to the Westward, ascended the Missouri with a trading party, on researches in botany and natural history. latterly he has been engaged with a manufacturing company to the Eastward. he is a very modest and learned man, and I believe of great worth."

The second letter was written to Bradbury himself.[2]

"MONTICELLO, Feb. 29, 16.

"*Dear Sir*

"Your letter has laid by me a month unacknowledged and unacted on, which should not have happened, had not an engagement in a business of peculiar pressure obliged me to suspend all correspondence till I got thro' it. (This probably refers to the translation of Senator Tracy's book on Political Economy into English and the

[1] Jefferson papers, Library of Congress, Series II, Vol. 23, No. 36.
[2] Jefferson papers, Library of Congress, Series II, Vol. 11, No. 126.

work of getting it into the hands of the printer. (R.H.T.). I have now written to the Secretary of War, expressing to him your wish and your fitness for the appointment of a Commissioner on the Arkansas road. I should be very glad indeed if either in a public or private capacity you should be able to give us an account of the natural history of the Arkansas & Red River country. Should your friends have sent you spare copies of the publication of your western discoveries, I should be gratified by a sight of them. Accept the assurance of my great esteem and respect.

"TH. JEFFERSON"

The copy of this letter found among the Jefferson manuscripts in the Library of Congress is endorsed on the back in Jefferson's handwriting, "ret'd from N. Y. because *not found.*"

Three months elapsed before Secretary Crawford replied.[1]

"WASHINGTON, 31st May, 1816.

"*My dear Sir:*

"Your letter recommending Mr. Bradbury was read during a serious indisposition with which I was afflicted in the course of the last winter, and has been mislaid, so that I am not able to refer to it more particularly. No service of the kind for which that gentleman was proposed has been contemplated by the government."

Thus Bradbury's projected road like his botanic garden went into the limbo of unrealized hopes and thus seems to have closed the correspondence between Bradbury and Jefferson.

The reason for the disappearance of Bradbury without awaiting the issue of his appeal is probably to be found in his departure for England. Perhaps the appropriation of his botanical results by Pursh had warned him of the advisability of getting the remaining results of his travels on record; perhaps a desire to see his family again led him to seek England. At all events, he returned to his home probably in February and stayed in that country long enough to see the first edition of his Travels through the Liverpool Press. The preface to this book is dated August 1, 1817, at Liverpool.

According to Hill, the publication and attempted sale of his Travels severely taxed Bradbury's slender resourses, and darkened his outlook. This darkness was further intensified

[1] Jefferson papers, Library of Congress, Series II, Vol. 23, No. 34.

by the feeling that his arduous labors had failed to receive any just appreciation, and in his dispondency he is said to have resolved to quit his native land forever. This doubtless seemed well nigh an impossibility to one in his financial straits, but while wandering through the streets of Liverpool one day, Bradbury met an American sea captain with whom he had formerly been acquainted. Astonished and touched at finding his friend so nearly destitute, the captain forthwith offered Bradbury and his family free passage on his boat. Again the Atlantic was crossed and Bradbury was not disappointed in his reception by his old acquaintances in America. His return probably took place late in 1817 or in 1818, since the preface to his second edition of his Travels, written by the hand of another, states that Bradbury returned to America shortly after the publication of the first edition and took up his residence in St. Louis. Here in 1819, he was found by Dr. William Baldwin, botanist to Long's Expedition then on its way up the Missouri. Several days were spent at this point to permit such arrangements to be made as were necessary for the long trip in contemplation.

While here, Baldwin met him and writes to a friend, William Darlington, in a letter dated June 11, 1819.[1]

> "The venerable Mr. Bradbury called on me yesterday, and spent the day. His company had the most exhilarating effect upon my health and spirits. In looking over my collection, I begged him to claim anything he found that might be his own. It turned out that a few, which I had marked for new, were known to him,—but he requested me to describe them: and observed that since Lambert had pirated from him his former collections, it was not his intention to publish independently,—and that he would with great pleasure, place in my hands all that he possessed, for publication:—and that he should continue to pursue Science for the intrinsic love he had for it,—and continue to furnish me with descriptions and specimens, to be published as I might see fit, under his name and authority. As this was the first interview, many inquiries, which I intended to make were omitted." . . .

A fortnight later, Baldwin again wrote to Darlington from St. Louis referring to a new genus of the *Leguminosae* which

[1] Darlington, William, "Reliquiae Baldwinianae," p. 316.

he thinks "ought prehaps to be called Bradburya." Plans for future botanical interchange between himself and Bradbury are indicated. These were not to be realized, however, since not many days later Baldwin succumbed to his old enemy, tuberculosis, and was buried at Franklin, besides the the Missouri. He did not live to perpetuate the name of Bradbury in the manner suggested. Baldwin and Bradbury were both probably unaware that this well earned tribute had already been paid by their colleague, Rafinesque.[1]

How long Bradbury remained in St. Louis is not clear, but this period could not have exceeded five years. Hill is authority for the statement that he ultimately became curator and superintendent of the Botanical Gardens at St. Louis where he was not only placed beyond the fear of penury but was honored and respected by the residents of the city. He is said by Hill to have continued his researches and investigations often receiving visits from the Indian chiefs whom he had met in the wilds.

Another glimpse of Bradbury who seems not long to have remained in St. Louis is obtained in a letter written by William Chambers to John S. Skinner, Editor of the *American Farmer*, an early agricultural journal published in Baltimore.[2] It is dated at Middletown, Kentucky, January 22, 1823, and after referring to Bradbury's explorations and the publication of his travels, the writer says: "Mr. Bradbury when on his tour through the United States became so much attached to our republican institutions, that he removed his family to this country, and is residing in this village."

For definite knowledge concerning the death of Bradbury, we are indebted to a short obituary notice sent by Judge Walter B. Douglas of St. Louis to Dr. R. G. Thwaites and kindly brought to my attention by Miss Annie A. Nunns of the State Historical Society of Wisconsin.[3]

"Died; at Middletown, Kentucky, on the 16th of March

[1] Rafinesque, C. S., "Flora Ludoviciana," 1817: 104.
[2] Chambers, William, "American Farmer," Baltimore, 4: 411, Mar. 21, 1823.
[3] Missouri *Republican*, Wednesday, May 7, 1823.

last, after a short illness, Mr. John Bradbury. Mr. Bradbury is known to the scientific world as among the first botanists and mineralogists. His knowledge in science generally was esteemed valuable. Never was there a better companion, nor a more sincere friend."[1]

Hill's belief that Bradbury's Travels deal with "the hitherto unknown solitudes which have since furnished the bulk of the cotton used in Lancashire" probably rests on a romantic ignorance of American geography.

[1] In view of this notice we may perhaps the more easily appreciate the poetic traits attributed to the chronicler of Stalybridge who relates a legend concerning Bradbury's last days. "In the spring of 1825 a strange desire took possession of Bradbury to revisit the haunts of the Red Men and he forthwith started from the city of St. Louis for that purpose. It may be that the trials of his early years had left their mark; it may be that his life was cut short by accident. Be that as it may, the last record of him states that he is supposed to have died, and been buried with great solemnity by the Indians somewhere in the valley at the head of the Red River."

THE CHEMICAL STIMULUS ESSENTIAL FOR GROWTH BY INCREASE IN CELL NUMBER

By FREDERICK S. HAMMETT

(*Read April 20, 1929*)

THERE are two chief types of cellular growth; Growth by increase in cell size, and Growth by increase in cell number.

Both of these are expressions of chemical processes.

The chemical factors in Mass Growth are pretty completely outlined at the present, at least in principle. The chemical processes concerned in growth by cell reproduction are awaiting discovery.

They are of the most fundamental and universal importance. They determine the quality, quantity, and nature of the food supply. They determine racial, familial, and genetic characteristics. When they go wild, destructive malignant growths occur. In fact the chemical processes concerned in growth by increase in cell number lie at the very root of human existence, human inter-relations, and human health.

The increase in deaths from malignant overgrowths, the thoughtless increase in human population with its high proportion of biological misfits, the decrease in areas available for food production, all these and more, make imperative an understanding of the basic chemical processes determinative of growth by cell reproduction. We can only direct when we know what we have to regulate.

Some years ago Dr. W. Blair Bell of England, reported results which indicated that the metal Lead retarded growth, and particularly the parts where growth by increase in cell number was predominant. His observations were not conclusive becaused they lacked detailed analysis. They were none the less valuable for they made a dent in what heretofore had been an unpenetrated barrier.

It occured to me that if Blair Bell's assumption be true, we might have in this metal a reagent wherewith the nature

of the compounds essential for growth by cell division might be ascertained.

But it was necessary first to prove that lead inhibits growth by cell division and specifically.

Using roots of seedlings grown in lead-containing culture solutions it was found that a lead-containing deposit is concentrated in the meristematic region, or region of active cell division. This is shown grossly in slide 1, and microscopically in free-hand sections in slides 2 and 3.

By suitable chemical means it was found that the lead within the cell is concentrated in the nucleus. This is shown in slide 4.

Now the histological work had indicated that many of the nuclei are fixed by lead and apparently in definite areas. A comparison of the distribution of the lead-fixed nuclei and the areas of active mitosis across a middle, longitudinal section of the root tip showed a beautiful correlation. The first slide gives a representative count on 11 control and 11 leaded roots. It is seen that in the central half the percentage mitosis in the normal root is twice as high as in the outer quarters; and that the number of lead-fixed nuclei is much higher in the central portion of the roots than in the outer quarters. In the next slide the relation is shown for corn by curves. Here also it is seen that rate of cell division and fixation of nuclei by lead run parallel.

These three sets of findings definitely establish the fact of an association between mitosis and lead deposition. They suggest that the lead deposit is a combination between the metal and some compound concerned in the process of cell division.

Growth studies with roots of seedlings grown in lead-containing culture solutions were made. The next slide gives figures of a representative experiment. The first column gives the initial root length values; the second those after 24 hours. The first, or NP_0 set, is the non-lead control. It is clear that root growth in length is retarded by lead. The next three slides are actual photographs of the response and they speak for themselves. The control lots are at the left.

In order to determine whether the roots were shorter because the cells were shorter, or whether they were shorter because fewer cells had been produced during the growth period studied, serial sections were made of the roots from these experiments and cell size measured and mitotic percentages computed from nuclear counts.

The next slide gives the results from 9 experiments in which cells in 8 control and 8 test roots were measured in each set. The figures show conclusively that the cells of the leaded roots were no smaller than those of their controls.

The next slide gives the percentage mitosis in roots from a representative experiment. The first column of each pair gives the values for the controls, the second those for the leaded specimens. They show conclusively that mitosis is inhibited by lead and increasingly with increasing concentrations.

From these results it is clear that growth is retarded because mitosis is inhibited, not because cell growth in size is adversely affected.

In order to be sure that the phenomenon is not confined to vegetable forms I have investigated the growth reaction of chick embryos to lead. It was found that here, as with the roots, differential inhibition was exhibited. Comparing embryos of the same number of somites it is seen that the development of the head end of the chick is markedly retarded. The leaded chick is to the left. Somite size is also less in the Pb-chicks. Since these are the regions of the most active growth by increase in cell number the differential inhibition is supportive evidence that lead specifically retards growth by cell division.

In view of the foregoing and the fact that a lead compound is found concentrated in the region of active cell division of the root tip, there developed the working hypothesis that mitosis is stopped by lead because a compound essential for cell division is removed from activity by the metal.

The next step was the analysis of the compound formed by lead. Without going into details it may be stated that it was

found by a variety of tests under the microscope and otherwise that the lead deposit is a combination between the metal and a sulfhydryl containing compound, analogous to, if not glutathione itself.

This finding, when correlated with the fact that sulfhydryl is found concentrated in the region of active cell division in the normal non-leaded root tip, is highly significant.

Let me summarize the data so far presented in order that a clear picture of the relations leading to the next conclusion may be had.

In the first place lead specifically inhibits mitosis. In the second place the nuclear activity of mitosis is associated with a lead fixation. In the third place sulfhydryl is concentrated in the mitotic areas as is the deposition of the metal. Further this lead compound is a combination of the metal with sulfhydryl. From this it is just to conclude that mitosis is inhibited because lead removes sulfhydryl from activity and the hypothesis naturally follows that growth by increase in cell number is importantly factored by sulfhydryl.

The next step, then, in the search for the nature of the chemical processes concerned in cell reproduction is the testing of this hypothesis. Such has been my work for the past year.

In order that those unfamilar with the chemical terminology may visualize what I am talking about, the next slide is given. On this the structural formulae of two simple sulfhydryl containing compounds thio-glycollic acid and cysteine, are compared with their non-sulfur-containing basis, acetic acid and alanine. Please note that sulfhydryl is the name given the $-SH$ group attached to the organic radical, and which in these particular compounds replaces hydrogen. It is the growth response to this group which has been studied.

Now since sulfhydryl is concentrated in the meristematic region of the normal root-tip, the first tests were made with acid extracts of this part, controlled by like extracts of the part just above where sulfhydryl concentration is decidedly less. This is shown in the diagram on the slide.

Acid extracts were used because the $-SH$ group is rela-

tively stable therein. From 2 to 4 hundred tips provided the extracts, which were tested on from 24 to 40 roots in each experiment. The next slide shows that acid extracts of the region of active cell division contain something stimulative of root growth in length.

The first column gives the percentage increment of the controls, the second that of the tests, and the third the difference which is always in favor of the tests.

Now $-SH$ is unstable in alkaline solutions. Therefore it was reasoned that if the growth stimulating effect exhibited in the acid extracts is $-SH$, no such activity should be exhibited when the extracts are made with an alkaline menstruum. Consequently a series of tests were run with $NaHCO_3$ extracts. The data are given on the same slide. It is seen that the expectations were realized, for no stimulation, in fact a trend to retardation, obtained.

The fact that acid extracts stimulate while alkaline do not, combine with the fact that $-SH$ is concentrated in the effective region, is supportive evidence for the hypothesis.

The next step was to test the action of pure sulfhydryl compounds. Thio-glycollic acid, Sodium and Potassium thio-glycollate, di-sodium-di-thio-diglycollate, cysteine and cystin were used and controlled by equivalent concentrations of like compounds minus sulfur. The optimal $-S$ concentration was found to be from 2 to 4×10^{-7}.
The next slide gives the root growth in length reaction of a few representative experiments. It is seen that stimulation is produced.

In other experiments roots were first inhibited by lead and then transferred to sulfhydryl containing cultures after thorough physiological washing. Here, too, stimulation of growth took place. The results of one or two of the many experiments are given on the same slide.

This stimulation of root growth in length by $-SH$ is no proof that cell division has been accelerated. This can only be had from determinations of cell length and percentage mitosis in the material used. These were made.

The next slide shows the average cell length in roots from the experiments just given. It is seen that cell length instead of being greater is uniformly shorter in the sulfhydryl fed roots. Hence the length growth stimulation is not due to cell size growth, but must be due to increase in rate of cell division. The next slide gives the percentage mitosis in the same roots. It is clear that cell division is greater in the sulfhydryl fed roots than in their controls. This is clean cut proof that sulfhydryl stimulates growth by increase in cell number specifically.

The results on the previously leaded roots are particularly significant. Here sulfhydryl was thrown out by lead and mitosis was inhibited. Then by replacement of the inactivated group cell division was again stimulated and root growth in length accelerated.

The fact that increased rate of cell division is accompanied by decreased cell size in the tests is further indirect proof for the conclusion that mitosis is stimulated by sulfhydryl, for a statistical analysis of these two phases of growth, using all the material, brought out the biological fact that cell size is smaller the greater the rate of cell reproduction, a phenomenon previously noted by Conklin and Richards. The correlation coefficient in my material is -0.577 ± 0.075.

I realized of course that the biological universality of the premise could not be established on plants alone. So I have tested the action of sulfhydryl compounds on reproduction rate in paramecium. Here concentrations were 10 to 100 times as dilute as with the roots. In these experiments all the organisms were counted, for it was found that the system of aliquot counting fails to give a true picture. The tests were controlled with like compounds minus the sulfur.

The next slide gives the results of a few of the many experiments. The first column gives the number of sets in each experiment, the second, the initial number of organisms in each set, the third, the average terminal number in the controls and the fourth, the average terminal number in the tests.

It is at once clear that in the animal as in the plant sulf-

hydryl is a stimulant of growth by increase in cell number. Here as in the plants also, it was noted but not measured, that where reproduction rate is enhanced cell size is less.

It might be stated in passing that Miss Sharpe in my laboratory has found the simplest sulfhydryl compound known, namely H_2S, also to be effective.

I also tested the action of three naturally occurring sulfur compounds, namely, glutathione from yeast, ergothioneine from blood, and crystalline insulin from the pancreas.

The glutathione was kindly furnished by Dr. James Johnson of the U. S. Public Health Service. It stimulated cell division in both plants and animals. Agreeing with this is the report by Dr. Lillian Baker of The Rockefeller Institute, that glutathione stimulates cell proliferation in tissue cultures of fibroblasts. My experiments differed from hers in that my tests were controlled by a mixture of alanine and glutanic acid.

The disulfide ergothioneine which was supplied by Mr. T. Swann Harding of the Experiment Station, Beltsville, Maryland stimulated cell division in roots but not reproduction in Paramecium.

Professor Abel of Johns Hopkins graciously gave the crystalline insulin. This had no effect whatsoever on cell division or growth as studied by my methods. This was not unexpected since the specimen used gave but a very, very faint nitroprusside test for sulfhydryl in either the reduced or oxidized form.

Apparently the sulfur linkages in insulin are either qualitatively or quantitatively less labile than in the usual run of sulfhydryl compounds.

The experiments presented in this report demonstrate decisively that sulfhydryl is the essential chemical stimulus for growth by increase in cell number.

There is a mass of impressive correlated data which gives incidental support to the belief. For long years it has been known that the sulfur amino acid cystin is essential for growth. Denny has reported that the sprouting of potato tu-

bers is enhanced by thio-urea. Baker got stimulation of cell proliferation in tissue cultures with glutathione. Voegtlin and Thompson and others have found high concentrations of sulfhydryl in tumor tissue where growth by increase in cell number is rampant. Giroud and Bulliard have found significant concentrations of sulfhydryl in the skin, an organ where replacement of tissue worn off by abrasion is continuously in progress by a process of cell increase.

Moreover trauma increases cell division as Haberlandt and Reiche have shown, and I have found that trauma causes liberation of sulfhydryl.

All these observations are consistent with the conclusion drawn from my experiments that sulfhydryl is specifically essential to cell proliferation.

We now step from the clear atmosphere of fact into the fog of interpretation where speculation is but a groping. We can say that the sulfhydryl oxidation-reduction system is essential for growth by increase in cell number because the oxygen demand of this process is great and the needs are peculiarly supplied by this chemical catalytic combination. But this is really begging the question. Not only do we not know why the oxygen demand of dividing cells is high, but even we are totally ignorant of where, how, and why sulfhydryl exerts its stimulating influence on the processes of cell reproduction. The solution of these problems rests on the knowledge of the chemical reactants comprising the processes of the phenomenon of cell division.

To assign to −SH− containing compounds the role of catalyst as distinct from food would be a mere juggling with words. The simple fact that protein-bound −SH exists and that cystin or cysteine is an integral part of the protein molecule is evidence enough that compounds of this type are fundamental constituents of the body substance and therefore to be considered as foods.

The important question to be solved is the order of events leading to the availability of SH as the stimulant to cell division. Of course practically all will agree that the ultimate

source is the sulfate of the soil, and that this being reduced and organically combined by the plants, serves as preformed sulfhydryl for animals which apparently are unable to synthesize the group, at least as a part of cystin, from sulfur or inorganic sulfates. But what the animal organism does to this save to store a part as protein-bound —SH, to exhibit a part as glutathione and thiasine, and to excrete a part as sulfate and neutral sulfur, is unknown. The available fragments of information are insufficient building material for any satisfactory concept.

With regard to the events related to the cell division stimulating property of sulfhydryl the following can be postulated, however, as a preliminary working hypothesis.

In living, as in non-living, forms each action is counterbalanced by a re-action, which, if effective, accomplishes that adjustment of the organism necessary for survival. The trend is always towards equilibrium, and as developed by L. J. Henderson and those who have followed him, elaborate chemical mechanisms exist for its maintenance. There is no reason to exclude the processes of cell division from this generality. In these there is every gross evidence that restrictive as well as stimulative factors are at work.

Since sulfhydryl is the essential chemical stimulant of cell division, it is conceivable that a chemical change in the group occuring as a result of its activity might well result in a compound possessing inhibitory properties. These would be exhibited when the inhibitory derivative accumulated in adequate concentration. In other words, the concept is that cell division is regulated by a chemical equilibrium in which sulfhydryl is the key group.

This is no radical departure, but is simply the extension of well accepted principles to additional fields.

Support for the idea is had from Harrison's observations that the oxidation of R—SH is catalyzed by R—S.S—R, and that the catalytic action of dithio-diglycollic acid is lost when the disulfide linkage is destroyed. He writes the following equations as possibly representative of the direction of the equilibrium changes:

$$R - S.S - R + H_2O \rightleftarrows R - SH + R - SOH$$
$$R - SOH + O \rightleftarrows R - SOOH$$

These are oxidation changes such as might be expected to occur in regions of active cell division where the O_2 consumption is high.

I too have observations which point in the same direction.

In the first place it was noted that whereas freshly prepared and freshly opened samples of the thio-glycollic acid gave good stimulation of root-length growth and reproduction rate in paramecium, samples which had been exposed to the air for some time were apparently less stimulative. The same was true of the Na-dithio-diglycollate. In fact this salt eventually became entirely unstimulative of root growth in length.

As has been noted earlier, both of these compounds lost their characteristic odor after exposure. Since all that had been done was to admit air to the preparations, it is tentatively concluded that oxidation destroys the effective sulfhydryl group. Moreover there is the fact that alkaline, hence oxidized, extracts of root tips retard instead of stimulate root growth in length, as shown earlier. Finally there is the fact that the semi-oxidized sulfhydryl of cystin is generally less stimulative than the reduced form of cysteine.

These data then are consistent in the showing a drift towards loss of stimulating power on oxidation. Such being the case a next step is to test the equilibrium hypothesis by a study of the action of the products of oxidation of the sulfhydryl group in cell proliferation. These studies are now being made as are others of allied import.

I shall not here go into any detailed exposition of the possibilities opened up by the results presented in this report. All that I will now point out is the hope that through their understanding and application we may ultimately be able to regulate normal and pathological growth by increase in cell number.

Summary

The development of this study has been a succession of steps each of which rests on the preceding. It falls naturally into three distinct stages. The first is that of:—

Identification.—Here it was found that the lead precipitate present in the meristematic region of root tips grown in Pb-containing culture solutions is a combination of lead with sulfhydryl. In such tips mitosis but not growth by increase in cell size is inhibited. Also it was found that sulfhydryl is concentrated in the meristematic region of normal roots. Therefore the hypothesis was developed that growth by increase in cell number is specifically factored by −SH. The next stage was the—

Testing of Extracts.—Here it was found that acid extracts of the meristem of root tips accelerated root length growth when controlled by acid extracts of the next distal portion, while alkaline extracts similarly controlled showed no such activity. This proved that the root region of highest sulfhydryl concentration and mitotic activity contains a naturally occuring acid-stable, alkali-labile substance stimulative of root growth in length. These findings are thus physiologically and chemically consistent with the hypothesis. The next stage was the—

Testing of Synthetic Compounds.—Here the action of a variety of sulfhydryl compounds on mitosis in root tips and reproduction rate in paramecium was studied, using the same compounds minus the sulfur moiety as controls. It was found that the −SH group stimulates cell proliferation in both plants and animals. Cell size growth is not stimulated.

Thus, through identification and testing of the identified group in natural and synthetic compounds, the conclusion is arrived at that—

Sulfhydryl is the essential stimulus to growth by increase in cell number.

A MOUNTED SKELETON OF PALAEONICTIS

By WILLIAM J. SINCLAIR and GLENN L. JEPSEN

(*Read April 20, 1929*)

THE Princeton Scott Fund Expedition of 1927 was so fortunate as to secure from the lower levels of the Gray Bull formation, lower Eocene, an associated skeleton of *Palaeonictis*, No. 13001 in the series of vertebrate fossils in the Geological Museum of Princeton University. This specimen, which was discovered by the junior author on the second of July, 1927, comes from low badlands bluffs paralleling the wagon road on the south side of the Gray Bull River, between Dorsey Creek and the old bridge at Otto, Bighorn Basin, Bighorn County, Wyoming. In places, it is invested with a heavy coating of hematite which it is quite impossible to remove except by grinding, and had suffered somewhat from decay and crushing previous to fossilization, the skull being disarticulated and the pieces flattened, but, despite these handicaps, it has been possible to prepare it for exhibition as a panel-mount, with a minimum of plaster reconstruction which is not self-evident in the accompanying photograph and will be indicated in detail in the description which follows. The very close agreement of the skeleton with that of *Oxyaena* fully justifies Dr. Matthew's inclusion of *Palaeonictis* in the Oxyaenidae, while the dental measurements, so far as comparison can be made, are almost identical with those indicated in Osborn and Wortman's splendid figure of the type of *Palaeonictis occidentalis*,[1] to which species our specimen is referred. The skeleton, the first so far found, as shown in Fig. 1, stands 16¾ inches high to the top of the scapula, and measures 52¼ inches between verticals placed at the front end of the skull and at the curve of the tail on the base floor. In color, most of the bone is a deep chocolate brown, diversified with patches of

[1] *Bulletin Am. Mus. Nat. Hist.*, Vol. IV, No. 1, Article XI, p. 104, Pl. IV, 1892.

dull red hematite and greenish-gray calcareous sandy matrix especially on the skull where the removal of all matrix by grinding did not prove practicable.

When found, the skull was in several pieces, some separated from each other in the matrix, others inverted and superposed, all heavily coated with hematite and crushed flat, making their assembling a matter of considerable difficulty. As reconstructed and seen in Fig. 1, the skull is assembled from five pieces: a large featureless expanse, the laterally crushed and conjoined brain case and atlas; a facial portion anterior and inferior to the right orbit, but not including the premaxillae; both nasals; and a part of the left maxilla. Less detail is visible in the facial region than is shown in Osborn and Wortman's figure of the type, but the shortness of the face anterior to the orbit, is confirmed. The lower and anterior border of the orbit, as it appears in the photograph, is entire; the upper border, including the postorbital process, is plaster reconstruction connecting the facial segment with the crushed brain case in the rear, with which the parts preserved did not make contact. The crushing of the brain case has thrust the side wall of the skull, appearing as the jagged dorsal profile in the photograph, above the level of a moderate sagittal crest which is entire and terminates in the lambdoidal crests at the point where the upper profile of the crushed skull drops downward at an angle toward the fused atlas and basicranium, both rendered featureless by the investing hematite. Anterior to the upper canine which is smaller than in the type specimen, perhaps a sex difference, the premaxillary border has been restored in plaster. What probably is a left premaxilla, from its position in the matrix with respect to the nasals, is both hematite-coated and crushed so flat that it could not be used in the reconstructed skull. The nasals, found in contact along their median suture, seem to be entire at their broadly-rounded free ends, but apparently lack most of the remaining margin and do not make contact with the facial segment, with which they have been connected by plaster.

As in the case of the type specimen, our representative

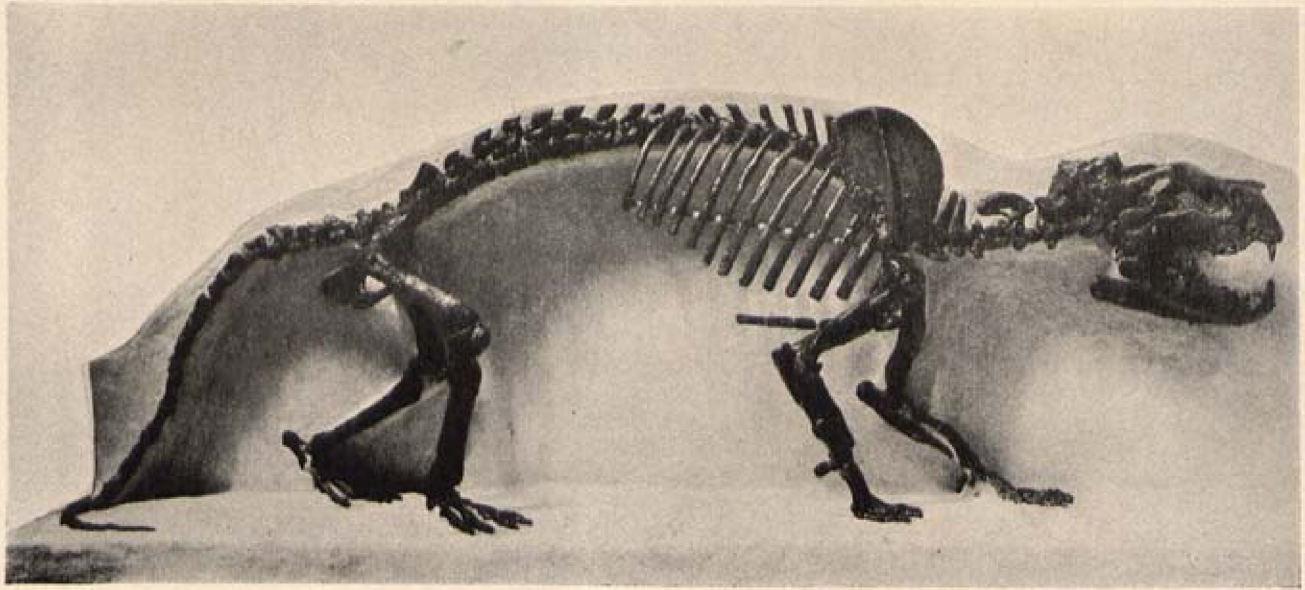

Fig. 1.—*Palaeonictis occidentalis*, mounted skeleton from the right side, No. 13001 Princeton University Geological Museum, about one-eighth natural size.

lacks a part of the mandibular symphysis which has been rebuilt and set with five small worn incisor teeth found loose in the matrix. From their size they are assumed to be lower incisors. Both angular processes are intact and prove to be considerably larger and rounder at the end, and to extend farther backward, than is suggested in outline in the figure of the type. Both lower canines are in place and also p. $\overline{3}$, $\overline{4}$; m. $\overline{1}$, $\overline{2}$ on either side. P$\overline{2}$, right, was found detached in the matrix and has been reset in its proper position. The dental series on the left, where less deformation has taken place, agrees closely with the dimensions of the type with which it is absolutely identical in all details of structure, so far as the parts preserved and the somewhat more worn condition of the teeth permit comparison. Since there are but two molars, both retaining the metaconid and the second a reduced copy of the first, there can be no question that our specimen has been correctly identified as *Palaeonictis*. It is referred to *P. occidentalis* on the basis of agreement, not only in dental pattern, but in dimensions also, especially in the lower series, since what remains of the upper dentition is almost completely submerged in hematite. The remaining details of lower jaw structure have already been presented by Osborn and Wortman [1] and do not need to be repeated.

Centra of the axis and the five remaining cervicals, all remarkably short, were recovered. All lack spines and most of them the transverse processes also. The centrum of the axis is short with a thick peg-like odontoid and a prominent inferior tubercle on the posterior inferior surface. The fourth cervical has the transverse process apparently entire and perforated for the vertebral artery, which undoubtedly indicates a similar perforation of the transverse processes of those preceding and following. A large hatchet-shaped piece which looks like that pertaining to the transverse process of the sixth cervical has been set in place, but does not make contact.

Ten dorsal vertebrae have the centra entire, but lack

[1] *Loc. cit.*, p. 96.

spines and transverse processes except on the tenth where the spine seems to be entire, but has so much hematite on it that we cannot see the condition of the tip. The exact position of some of the vertebrae, in the series from one to ten, is difficult to determine from the centra alone and mistakes may have been made in their emplacement. The anticlinal vertebra has been restored in plaster, and, back of that, an entire and a broken centrum have been introduced into the dorsal series, making the total number thirteen, but there might have been another one depending on the interpretation placed upon certain hematite-coated pieces unused in the mount. Seven vertebrae with longer centra than the rest and lacking spines and transverse processes, but with no trace of articular surfaces for rib-heads have been interpreted as lumbars and arranged in the order of their size, decreasing forward. They seem to lack metapophyses, but anapophyses are present in the anterior part of the lumbar and posterior part of the dorsal series. At least one of the lumbar post-zygapophyses is revolute, longitudinally and shallowly grooved above and cylindrical below. Spines have been restored; transverse processes omitted.

The anterior sacral centrum is preserved and two others have been added from analogy with *Patriofelis*. The contact with the ilium seems to be confined mostly, if not entirely, to the first one which retains its articular surface on the left side. The tail was long and stout at the base as shown by the size of the proximal caudals. Of the twenty-five caudals which appear in the mount, eighteen were recovered either entire or in part, and several more could have been added. No recognizable chevrons were found.

Despite the many rib fragments not a single one of either side was entire or could be pieced together. We have, therefore, been forced to use pieces which may not have had the position given to them in the series. Five short sternebrae were found. Another has been added and there may have been more.

The left scapular head and a somewhat larger fragment

of the corresponding element on the opposite side are preserved. Evidently these bones had decayed previous to fossilization, as the parts recovered were completely submerged in the sandy clay stratum in which the skeleton lay. On the right scapular fragment, the axillary border is entire for about half its length above the neck; anteriorly, above the suprascapular notch, the border is entire throughout an extent of about twenty-five millimeters; beyond these points it has been restored. On the right side the coracoid process and most of the spine are lacking, except for a flattened hematite-covered projection which may be the metacromion. As restored, the scapula has been given the general outline employed by the American Museum in its mounted skeleton of *Oxyaena forcipata* and may be too large and set too high in our *Palaeonictis*. No attempt has been made to model the details of the acromion process for lack of comparative material, but the coracoid process has been reconstructed from the opposite side where it is entire and prominent.

The humerus is stout, very broad at the distal end and of about the same relative length as in *Oxyaena*. The deltoid crest is well marked, but not excessively prominent and occupies rather more than half the length of the shaft. There is an extremely prominent internal epicondyle; an entepicondylar, but no supratrochlear foramen; and a rather inconspicuous supinator ridge. The shaft is straight, the head pyriform, the tuberosities well developed, but not especially prominent, and the bicipital groove moderately deep.

The ulna has a large globular olecranon process, well shown on the left hand element in the photograph, and a short shaft, rather convex on the inferior border and broadly grooved lengthwise, internally. Both ulnae lack distal ends and on the right element the olecranon had either decayed previous to fossilization or failed to develop normally, for the outcurving process is shorter and has a different configuration from its opposite. The hematite coating, however, prevents an inspection of the actual surface of the bone.

The radius is a stout straight element with a short shaft,

widening distally. The anterior convexity seen in the shaft of the left radius is due to crushing, that on the opposite side being much straighter. The head is a flattened oval and, apparently, had its rotation considerably limited thereby.

The fore foot has the same general plan as in *Oxyaena* and *Patriofelis*, is pentadactyl, mesaxonic, with separate scaphoid and lunar. These two carpal bones, and the pisiform, are present on either side, and, in the left carpus, what is probably the cuneiform has been set in place, but the other elements in either fore foot have been outlined in plaster following the gen-

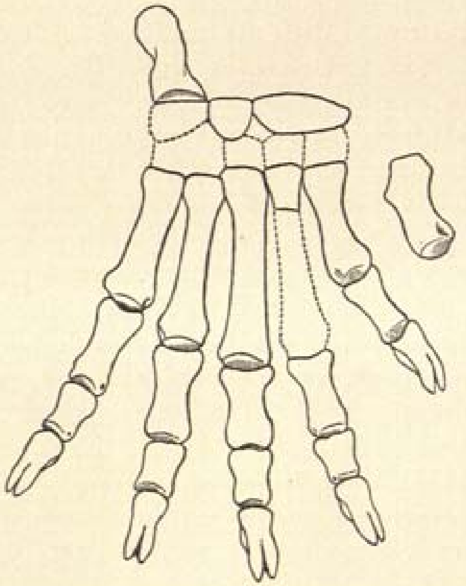

Fig. 2.—*Palaeonictis occidentalis*, outline drawing of the right fore foot, one-half natural size, showing extra lateral metapodial found associated with the skeleton.

eral plan of the carpus in *Oxyaena*. We are reasonably certain that several carpal elements are present among the fragmentary and unused materials, not included in the mount, but if so they are mere lumps of hematite which are quite impossible to articulate. The first metacarpal (Fig. 2) is the shortest of the lot, and if the additional metacarpal? shown in Fig. 2 pertained to the front foot there may have been a supernumerary first digit. No contact for it has been found on any

of the podial elements recovered, but it seems to pertain to this skeleton. The phalanges of the first and second row are broader than in the hind foot and some of them are a little longer. The unguals, also a little larger than in the hind foot, are short, wide and deeply fissured. Metacarpals 1,3, 4, 5 right with the proximal end of 2 and 1, 3, 4, 5 left were recovered, also most of the phalanges of the first row, several of the second row and two unguals supposed to pertain to the fore foot. The rest have been cast or modeled. The foot is broad and spreading and has been given a plantigrade pose on the left side which is carrying the weight as the animal is about to step forward with the right fore. Objection may be made to the amount of clearance given the carpus and metacarpus on this side and also to the extreme plantigrade position of the left fore. A better approximation to the correct position might have been attained had there been more material with which to work and less hematite on the articular surfaces.

In the pelvis, the ilia are entire except for the right iliac crest which has been restored from the opposite side. The bodies of both ischia are practically complete, but their descending rami, except for one implanted fragment, and the pubes have been restored. The ilium is much less expanded than in *Patriofelis*, but, nevertheless, develops a thin concave plate above the bar, widening anteriorly. The distal end of the bar is everted but little beyond the acetabular plane.

The femur is a stout bone proportionately shorter than this element in the American Museum's mounted skeletons of *Oxyaena* and *Patriofelis*, with a heavy flattened shaft, stout greater trochanter well separated from the head and not rising above it, the lesser trochanter small and internally placed and the third trochanter, about one third of the way down the shaft from the proximal end, indicated by a prominent bulge for muscular attachment. Both femoral shafts have required considerable plaster patching, as they were in separated pieces. The patellar trochleae seem considerably larger than necessary for the rather small patellae, both of which were

recovered, one quite free from hematite. These trochleae are broadly excavated. The condyles are, proportionately, much as in *Oxyaena*, moderately deep anteroposteriorly and strong.

The tibia is short with the cnemial crest not especially long or prominent, so that the cross-section at the middle of the shaft is round-oval. The internal malleolus is thick and heavy and the surface for articulation with the astragalus almost flat and inclined obliquely inward. The left tibia is

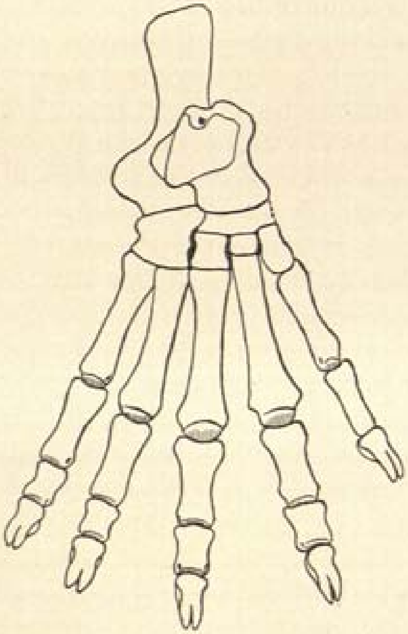

FIG. 3.—*Palaeonictis occidentalis*, outline drawing of the right hind foot, one-half natural size.

entire, but the right, except for the proximal end, has been modeled in reverse from the opposite element.

The straight fibular shaft has about the diameter of an average wooden leadpencil and is entire on the right side except for two short gaps. Distally it has a large oblique facet for the astragalus facing internally and a small terminal contact with the calcaneum.

The hind foot (Fig. 3) has the metapodials and phalanges

slightly more slender than those of the fore foot and, like it, is mesaxonic and spreading. From the extremely flat astragalar trochlea it is believed to have been plantigrade. The calcaneum is short with stout tuber calcis, prominent peroneal process separated dorsally from the body of the bone by a broad longitudinal groove, very oblique cuboidal facet and a rather undefined area for contact with the fibula. The astragalus (Fig. 4) is wide and short with an almost flat tibial facet having no prolongation on the neck and of rather limited backward extent where it is deeply notched by the groove lodging the astragalar foramen. The neck is broad transversely and the head broader still, but flattened from above downward and having considerable contact with the cuboid which is a short element as in the Oxyaenidae generally. No navicular on either side was recovered. If the bones with which it articulated have been correctly placed, it could not have had much thickness in proportion to its breadth. The cuneiforms are all short, the entocuneiform being the largest and longest of the lot. The metatarsals are short, with but little inter-locking. Distally, as is also true of the metacarpals, they have spherical heads, keeled on the plantar surfaces Fig. 5). The phalanges, like those of the fore foot, are also short and probably were a little less robust than the latter, especially the terminal claws.

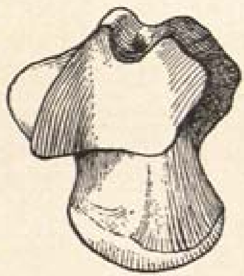

FIG. 4.—*Palaeonictis occidentalis*, right astragalus, dorsal aspect, natural size.

FIG. 5.—*Palaeonictis occidentalis*, distal end of right metatarsal II, natural size.

In all details, the skeleton of *Palaeonictis* is typically oxyaenid and there can be no question of the propriety of placing this genus in the Oxyaenidae rather than in a family of its own. We have nothing to add to Dr. Matthew's able discussion [1] of the affinities of this family and of the adaptive significance of the oxyaenid skeleton.

[1] Matthew, W. D., "The Carnivora and Insectivora of the Bridger Basin, Middle Eocene," *Memoirs, Am. Mus. Nat. Hist.*, Vol. IX, Part VI., p. 409, 1909.

Palaeonictis was, evidently, a long, low, powerfully built creature with strong loins and long thick tail, carnivorous with perhaps considerable bone-crushing ability, terrestrial-ambulatory in adaptation and plantigrade in gait. Two species have so far been recognized: *Palaeonictis gigantea* de Blainville[1] from the Suessonian (Lower Eocene) of France and *P. occidentalis* Osborn and Wortman from the Gray Bull Lower Eocene of the Bighorn Basin, Wyoming. The genotype is based on an incomplete mandible carrying on the right side $p\bar{4}$, part of $m\bar{1}$ and $m\bar{2}$ entire, supplemented by a fragment of the right ramus of a smaller individual with $p\bar{3}$-$m\bar{2}$ complete, and is from the lignites of Mirencourt, two leagues from Noyon in the Soissonais, France. It is especially remarkable for the great elevation at the end of the dental series ("par sa grande hauteur à la fin de la ligne dentaire"), its exterior convexity, its interior flattening, and by the manner in which the strongly convex inferior border is excavated slightly on approaching the symphysis. It is in other respects moderately elongated, very little curved on the upper or alveolar border and has but a single mental foramen externally beneath the point of the third premolar (free translation from the French text). In none of these respects is *P. gigantea* especially different from the American form. Of the two molars, the anterior, which is partly broken, was a little larger than the second, the crown of which is entire and "est formée d'une partie anterieure soulevée a trois points, et d'une posterieure en talon crénelé sur ses bords reléves, surtout en arrière." In de Blainville's figure of the right half of the mandible of his type the distance from $p\bar{3}$-$m\bar{2}$ on the alveolar border is 45.5 mm., and in his smaller supplementary specimen 41.5 mm. In our Princeton specimen of *P. occidentalis* this distance is 50 mm., and in the figure of Osborn and Wortman's type 49 mm., as it is, also, in their original specimen. If de Blainville's figures are correctly drawn to actual-size

[1] H. M. Ducrotay de Blainville, "Ostéographie ou description iconographique comparée du squelette et du système dentaire des cinq classes d'animaux vertébrés recent et fossiles pour servir de base à la zoologie et à la géologie," Vol. 2, Carnivora, N. Des Viverras, p. 79, 1842. Atlas (on the genus Viverra), Pl. XIII, *V. gigantea.*

scale, then $m\bar{2}$ is larger in proportion to $m\bar{1}$ than in the American form and has a tricuspidate heel, whereas a single-cusped heel is found in the latter, as distinctly emphasized by Osborn and Wortman in the characterization of their type. Presumably the genus is of Old World origin as *P. gigantea* is somewhat less advanced in the reduction of $m\bar{2}$, both as regards the relative size of this tooth and the cuspidation on its heel than is *P. occidentalis*. According to Professor Osborn's correlations, both species are of approximately the same geological age.

A few additional measurements are appended, which will aid in scaling other parts shown in Fig. 1:—

Restored skull, length, premaxillae to condyle of atlas, inc.	267	mm.
Lower jaw, length, ant. base of canine to angular process, inc.	167	"
Lower jaw, depth below $m\bar{1}$	29	"
Lower jaw, length, $p\bar{3}$–$m\bar{2}$	50	"
Humerus, length, greater tuberosity to edge of ulnar trochlea	164	"
Radius, length	104	"
Pelvis, length	192	"
Right femur, length, increased by crushing	177.5	"
Tibia, length, internal tuberosity to internal malleolus, inc.	147.5	"

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GUDERMANNIANS AND LAMBERTIANS WITH THEIR RESPECTIVE ADDITION THEOREMS

By ARTHUR E. KENNELLY

(Read April 19, 1929)

A GUDERMANNIAN, named after the German mathematician C. Gudermann, is well known to be any circular angle, say β, in definite functional relation to a hyperbolic angle θ, such that

$$\sin \beta = \tanh \theta, \tag{1}$$

$$\cos \beta = \operatorname{sech} \theta, \tag{2}$$

and hence

$$\tan \beta = \sinh \theta, \tag{3}$$

$$\cot \beta = \operatorname{csch} \theta, \tag{4}$$

$$\sec \beta = \cosh \theta, \tag{5}$$

$$\csc \beta = \coth \theta. \tag{6}$$

The preceding relations are contained in the statement that the circular angle β is the gudermannian of the hyperbolic angle θ, or

$$\beta = gd\, \theta. \tag{7}$$

The angle θ may be expressed in hyperbolic radians or "hyps"; while the gudermannian may be expressed in any circular units, such as sexagesimal degrees, circular radians, quadrants, or their decimals.

The gudermannian relation expressed by (7) is not limited to real angles. It is known that, in the general case, β and θ are complex, and

$$\beta_1 + i\beta_2 = gd(\theta_1 + i\theta_2). \tag{8}$$

Here β_2, an imaginary circular angle, has the properties of a hyperbolic real angle; while θ_2, an imaginary hyperbolic angle, has the properties of a circular real angle.

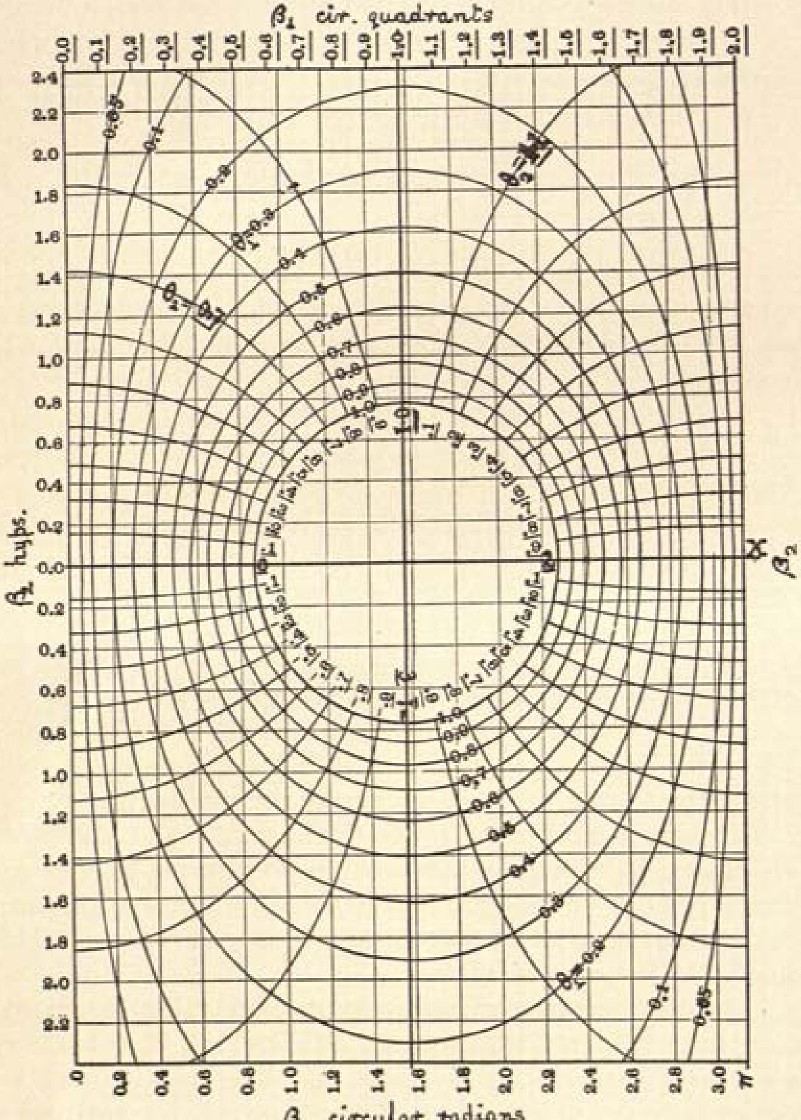

FIG. 1—Graph of Complex Gudermannians and Lambertians.

Graphs of Complex Gudermannians

Fig. 1 is a graph of the complex gudermannian relation (8), for θ_1 between 0 and 1.0, by steps of 0.1, and for θ_2 between 0 and 2π radians or 4 quadrants, by steps of $\underline{0.1}$ quadrant, or 10 grads, or 9 degrees. Since the graph is periodic or repetitive for θ_2, with a period of 2π radians, 4 quadrants, or 360°, it is clear that the graph virtually includes all values of θ_2 from 0 to $\pm\infty$.

Curves of the same geometrical species as those of Fig. 1 have been presented by other writers, in physics and electrical * engineering; but it is believed that these are the first of the kind to be presented in coördinate form as gudermannians.

To find the real circular angle β_1 which is the gudermannian of the real hyperbolic angle $\theta_1 = 0.5$, we enter the diagram of Fig. 1 at the origin of coördinates on the X axis, and move along this axis in the positive direction until we meet the loop of $\theta_1 = 0.5$, at $\beta_1 = 0.480$, which is the required gudermannian read off to radian measure along the scale below the diagram, and to quadrant measure along the scale above.

Again, to find $gd(0.5 + i\underline{0.5})$ where θ_2 is $\underline{0.5}$ quadrant, or 0.7854 radian, or 45°, we enter the diagram, as before, at the origin of coördinates, move positively along the OX axis, until the loop of $\theta_1 = 0.5$ hyp. is met, and then advance clockwise around this loop as far as the intersection of the radial curve marked $\underline{0.5}$. This point referred to the rectilinear coördinate background reads $0.635 + i0.737$; *i.e.*, $\beta_1 = 0.635$ circular radian on the horizontal scale at the bottom, or $\underline{0.404}$ in quadrant measure on the horizontal scale at the top. The imaginary component β_2 is 0.737 hyp.

Fig. 2 is a similar complex gudermannian graph, in accordance with equation (8), between the limits of $\theta_1 = 1.0$ and $\theta_1 = 4.0$, by steps of 0.25. Thus, $gd(1.5 + i\underline{1.5})$ is found to be $1.891 + i0.310$ circular radians, of which the real component is 1.891 radians or $\underline{1.203}$ quadrants, or 108° 20′ and the imaginary component is 0.310 hyp.

* Bibliography 5 and 6.

From a geometrical point of view, it may be noted that the graph of $gd(0 \pm i\theta_2)$ is an infinitely long straight line passing through the origin and coinciding with the axis of imaginaries. As we introduce θ_1 into the hyperbolic angle θ, the graph changes into a long vertical loop, with its major

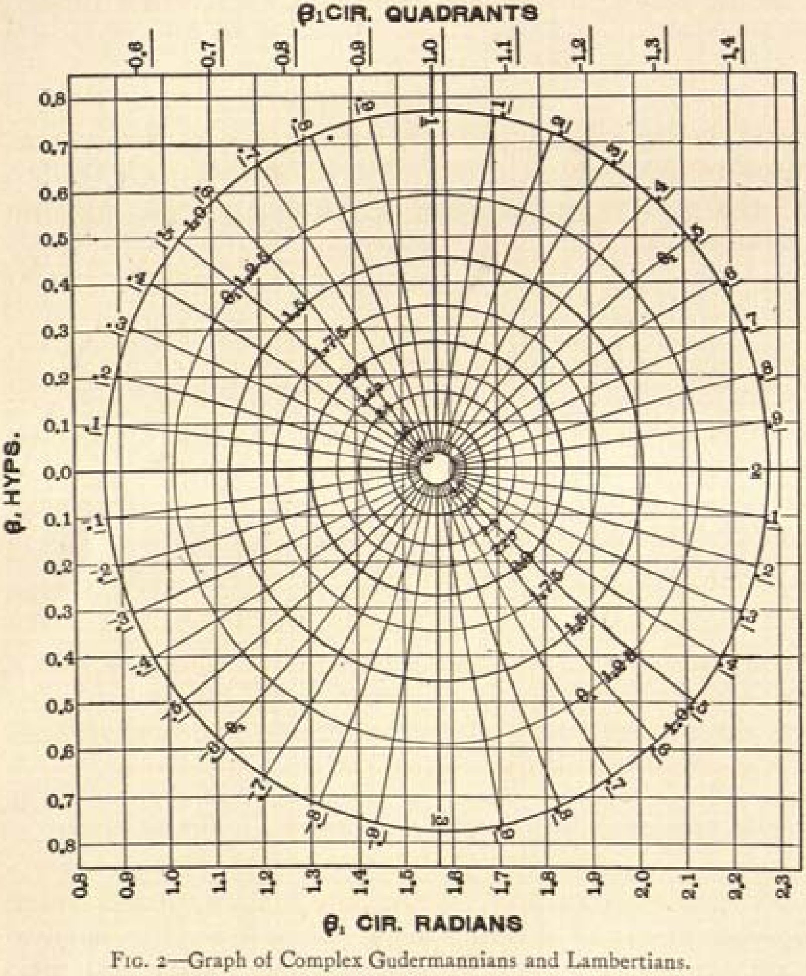

FIG. 2—Graph of Complex Gudermannians and Lambertians.

axis passing through the point $\pi/2$ or $\underline{1}$ quadrant, on the axis OX. As θ_1 is increased, the loop shortens, while remaining symmetrical to the $\underline{1}$-quadrant point, which becomes the

center of the diagram. When θ_1 exceeds say 4, the loop becomes a close approximation to a circle.

The graphs in Figs. 1 and 2 are mere outlines. Tables of $gd(\theta_1 + i\theta_2)$ are being computed to five decimal places, between 0 and 4 in θ_1, by steps of 0.05 and between 0 and 4 in θ_2, by steps of $\underline{0.05}$, for future publication.

Lambertians

If we seek the inverse function of (8); *i.e.* the anti-gudermannian[1] or *lambertian* of a complex circular angle $\beta = \beta_1 + i\beta_2$, we have the relation:

$$\text{lam}\,(\beta_1 + i\beta_2) = gd^{-1}(\beta_1 + i\beta_2) = \theta_1 + i\theta_2 \quad \text{hyps} \angle. \qquad (9)$$

To find the lambertian of a complex angle $\beta_1 + i\beta_2$ circular radians, with the aid of such a chart as Fig. 1, it is evident from the direct use of the chart for finding gudermannians, as already described, that we enter it with the coördinates $\beta_1 + i\beta_2$ on the rectilinear background, and then read off the point so fixed upon the curvilinear coördinates θ_1 and θ_2, bearing in mind that θ_2 is thus evaluated in quadrant measure, and may have to be dequadranted, in order to meet the needs of circular measure. Thus,

$$\begin{aligned} \text{lam}\,(1.029 + i0.495) &= 1.0 + i\underline{0.5} \\ &= 1.0 + i0.785 \quad \text{hyps} \angle. \qquad (10) \end{aligned}$$

Charts of the type indicated in Figs. 1 and 2, when developed in sufficient detail, become available for determining the values of both gudermannians and lambertians to a certain moderate degree of precision.

Derivation of Lambertians from Gudermannian Charts by Exchange of Axes.

It is well known that if we prepare a chart of the complex sine function of a hyperbolic angle θ according to the relation

$$\sinh\,(\theta_1 + i\theta_2) = u + iv \qquad (11)$$

[1] The writer is indebted to his colleague, Prof. E. V. Huntington, who has already employed lambertians, for the use of this term to designate the anti-gudermannian function, in honor of Johann Heinrich Lambert (1718–1777), the German mathematician who is credited with introducing hyperbolic functions into trigonometry. (Encycl. Britt.)

we obtain a family of confocal ellipses and hyperbolas. By marking off the ellipses in successive steps of θ_1 and the orthogonally intersecting hyperbolas in successive steps of θ_2 quadrant measure, the chart[1] may be used to evaluate $\sinh(\theta_1 + i\theta_2)$ with the rectangular coördinate background for $u + iv$. The same chart may then be used for evaluating $\cosh \theta$ by rotating it through 90°, thereby exchanging the X and Y axes. Similarly, if we rotate the gudermannian chart of Fig. 1 (or Fig. 2) counterclockwise 90°, the X and Y axes will be interchanged and the loops will be made horizontal. The loops will continue to carry the same numbers; but will be read off as β_2 instead of θ_1. The intersecting radial curves will also be changed from θ_2 to β_1. They will continue to be marked off in quadrant measure; but they will advance counterclockwise, starting from the lowest point of each loop as zero, on the new Y axis. By thus changing axes, and direction of rotation in the imaginary quantity, together with the interchange of θ_1 with β_2 and θ_2 with β_1, the same chart can be used to evaluate directly either gudermannians or lambertians. In other words, a chart of complex lambertians is geometrically the same as a chart of complex gudermannians covering the same range; but the loops in the former are horizontal, while in the latter they are vertical. There is also an interchange of symbols and of direction of advance around the center.

Addition Theorem for Gudermannians

The following relation, which is possibly new, will be found to hold for any two hyperbolic angles θ_1 and θ_2, real, imaginary or complex, and two corresponding general circular angles:

$$gd(\theta_1 \pm \theta_2) = \beta_1 \pm \beta_2 = \tan^{-1}\left(\frac{\sinh \theta_1}{\cosh \theta_2}\right) \pm \tan^{-1}\left(\frac{\sinh \theta_2}{\cosh \theta_1}\right) \quad \text{cir. radians.} \qquad (12)$$

[1] Bibliography 4.

Thus, if $\theta_1 = 0.4$ and $\theta_2 = 0.6$, hyps.; then with the upper sign of (12),

$$\begin{aligned} gd(0.4 + 0.6) &= gd(1.0) \\ &= \tan^{-1}\left(\frac{\sinh 0.4}{\cosh 0.6}\right) + \tan^{-1}\left(\frac{\sinh 0.6}{\cosh 0.4}\right) \\ &= \tan^{-1} 0.34649 + \tan^{-1} 0.58891 \\ &= 0.33354 + 0.53223 \\ &= 0.86577, \end{aligned}$$

which agrees with the tabular value of $gd(1.0)$.

From (12), we obtain

$$gd(0 - \theta) = gd(-\theta) = -gd\,\theta \pm 2n\pi \quad \text{cir. radians } \angle. \qquad (13)$$

Again

$$gd(\theta_1 \pm i\theta_2) = \beta_1 \pm i\beta_2, \qquad \text{cir. radians } \angle. \qquad (14)$$

where

$$\begin{aligned} \beta_1 &= \tan^{-1}\left(\frac{\sinh \theta_1}{\cosh i\theta_2}\right) \\ &= \tan^{-1}\left(\frac{\sinh \theta_1}{\cos \theta_2}\right) \quad \text{cir. radians } \angle. \qquad (15) \end{aligned}$$

and

$$\begin{aligned} i\beta_2 &= \tan^{-1}\left(\frac{\sinh i\theta_2}{\cosh \theta_1}\right) \\ &= \tan^{-1}\left(\frac{i \sin \theta_2}{\cosh \theta_1}\right) \\ &= i \tanh^{-1}\left(\frac{\sin \theta_2}{\cosh \theta_1}\right) \end{aligned}$$

or

$$\beta_2 = \tanh^{-1}\left(\frac{\sin \theta_2}{\cosh \theta_1}\right). \qquad (16)$$

From the last formulas, the gudermannians of complex hyperbolic angles may be derived. The Tables from which Figs. 1 and 2 have been plotted, were derived in this way. Also by (16)

$$gd(i\theta_2) = i \operatorname{lam} \theta_2. \qquad (17)$$

From (14), we also obtain directly:

$$gd\left(\theta \pm i\frac{\pi}{2}\right) = \frac{\pi}{2} \pm i \tanh^{-1}(\operatorname{sech}\theta) \tag{18}$$

and

$$gd(\theta \pm i\pi) = \pm\pi - gd\,\theta. \tag{19}$$

The last two equations may be checked by an inspection of Figs. 1 and 2.

Finally, if in (12) we make $\theta_1 = \theta_2 = \theta$, it follows that

$$gd(2\theta) = 2\tan^{-1}(\tanh\theta), \tag{20}$$

which is a known[1] relation.

Addition Theorem for Lambertians

Developing (9), we obtain:

$$\begin{aligned}\operatorname{lam}(\beta_1 \pm \beta_2) &= gd^{-1}(\beta_1 \pm \beta_2) = \theta_1 \pm \theta_2 \\ &= \tanh^{-1}\left(\frac{\sin\beta_1}{\cos\beta_2}\right) \\ &\qquad \pm \tanh^{-1}\left(\frac{\sin\beta_2}{\cos\beta_1}\right) \quad \text{hyps } \angle. \end{aligned} \tag{21}$$

This relation holds for all values of β_1 and β_2, real, imaginary or complex.

Thus, if $\beta_1 = 0.3$ and $\beta_2 = 0.2$, taking the upper sign in (21),

$$\begin{aligned}\operatorname{lam}(0.5) = gd^{-1}(0.5) &= \tanh^{-1}\left(\frac{\sin 0.3}{\cos 0.2}\right) + \tanh^{-1}\left(\frac{\sin 0.2}{\cos 0.3}\right) \\ &= 0.31120 + 0.21104 \\ &= 0.52224 \qquad \text{hyp.}\end{aligned}$$

which agrees with tabular values of lambertians or antigudermannians.

From (21) we have also

$$\operatorname{lam}(0 - \beta) = \operatorname{lam}(-\beta) = -\operatorname{lam}\beta \quad \text{hyps } \angle. \tag{22}$$

Again

$$\operatorname{lam}(\beta_1 \pm i\beta_2) = \theta_1 \pm i\theta_2 \qquad \text{hyps } \angle, \tag{23}$$

[1] Bibliography 2.

where

$$\theta_1 = \tanh^{-1}\left(\frac{\sin\beta_1}{\cos i\beta_2}\right)$$

$$= \tanh^{-1}\left(\frac{\sin\beta_1}{\cosh\beta_2}\right) \quad \text{hyps} \quad (24)$$

and

$$i\theta_2 = \tanh^{-1}\left(\frac{\sin i\beta_2}{\cos\beta_1}\right) = \tanh^{-1}\left(\frac{i\sinh\beta_2}{\cos\beta_1}\right)$$

$$= i\tan^{-1}\left(\frac{\sinh\beta_2}{\cos\beta_1}\right)$$

or

$$\theta_2 = \tan^{-1}\left(\frac{\sinh\beta_2}{\cos\beta_1}\right). \quad \text{cir. radians} \quad (25)$$

From (21) we also obtain directly (n being any odd integer)

$$\text{lam}\,(\beta \pm n\pi) = -\,\text{lam}\,\beta \quad \text{hyps} \angle \quad (26)$$

and

$$\text{lam}\left(\beta + n\frac{\pi}{2}\right) = \tanh^{-1}(\sec\beta) + i\frac{\pi}{2} \quad \text{hyps} \angle; \quad (27)$$

also

$$\text{lam}\,(\beta \pm 2n\pi) = \text{lam}\,\beta \quad \text{hyps} \angle. \quad (28)$$

Finally, if in (21), we make $\beta_1 = \beta_2 = \beta$, we find

$$\text{lam}\,(2\beta) = 2\tanh^{-1}\tan\beta \quad \text{hyps} \angle, \quad (29)$$

which is a known [1] formula.

Also, by (23),

$$\text{lam}\,(i\beta_2) = i\,gd\,\beta_2 \quad \text{cir. radians} \quad (30)$$

Periodic Repetition of the Graphs

Although the diagrams Figs. 1 and 2 may suffice to evaluate approximately $gd\,\theta$ and lam β, yet each diagram may be regarded as covering but a small area of the total graph. Fig. 1, for example, should be extended to $\pm\infty$ along the axis of Y. Its width along the axis is only π circular radians or 2 circular quadrants. We may, however place any number N of sheets like Fig. 1 side by side, with their X axes in com-

[1] Bibliography 2.

mon, and so produce a graph covering a total range of $N\pi$ radians, or $2N$ quadrants, in β_1 Each alternate sheet will have negative signs applied to the values of θ_1 on its loops; while the numerical values of θ_2 along the radial curves will be continuous over the junctions. In this sense, the graph extends to infinity in each direction along both X and Y axes.

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SOME EDUCATIONAL VALUES OF THE AMERICAN REVOLUTION

By EVARTS B. GREENE

(*Read April 20, 1929*)

In the summer of 1774, one distinguished New England delegate to the Continental Congress was already thinking of that revolutionary assembly in terms of education. "I suppose," wrote John Adams to his friend, James Warren, "you sent me there to school. I thank you for thinking me an apt scholar or capable of learning."[1] Adams was more self-conscious, or more articulate, than most of his contemporaries; but the educational processes which his letters and diaries record were not altogether unique.

One does not ordinarily think of the elder Adams as particularly teachable; but he was then in a receptive mood and fortunately had a candid friend in the person of Joseph Hawley, country lawyer and Congregational deacon of Northampton, Massachusetts. Like other Connecticut valley men of his time and ours, Hawley was not an undiscriminating admirer of Bostonians. So he observed that "the Massachusetts gentlemen," and more particularly the Boston men, were, in the opinion of some persons, "apt, from an inward vanity and self-conceit, to assume big and haughty airs." It might be well to keep this criticism in mind at Philadelphia. The New Englanders would probably meet there gentlemen "fully equal to yourselves or any of you, in their knowledge of Great Britain, the colonies, law, history, government, commerce, etc." Connecticut had its "very sensible, ingenious, solid men." There was ability also among the New Yorkers; and as for the provinces to the Southward, they doubtless had "men of as much sense and literature as any we can or ever could boast of." For the benefit of one-hundred-percent New

[1] Adams, "Works," IX, 338–340.

Englanders, proud of their English descent, Hawley called particular attention to the need of getting on with persons of "Dutch, or Scotch, or Irish extract." "For as of every nation and blood, he that feareth God and worketh righteousness, is accepted of him, so they ought to be of us." [1]

Reflecting on such matters as these, Adams suggested that Congress might well be conceived as a "University" in which succeeding groups of students might be received from year to year and suitably trained.[2] In order to profit by these advantages, however, preparation was needed, and Adams wished that he might have time to furbish up his "old reading in Law and History," so that he might "appear with less indecency" among colleagues whom he felt to be, in many ways, his superiors.[3]

The curriculum which our student was now beginning proved to be more extended than he, or any of his colleagues, could have realized, with exercises in economics and politics which became increasingly complex. Presently, under pressure from the British army, Congress went travelling, first to Baltimore and later, by way of Trenton, Easton, Bethlehem, and Reading, to Lancaster and York. All this, to use the language of a later Adams, was education. It was education also when a few promising scholars, like Adams himself, were sent abroad to continue their studies in foreign capitals. How well these opportunities were used is another question not so easily answered. Much of the story may now be read in the admirable "Letters of Members of the Continental Congress," edited by Mr. Burnett and published by the Carnegie Institution in Washington. For the present, however, let us see how the experience worked out in the case of our New England student.

Unfortunately the teachable attitude of Adams's early letters was not consistently maintained, as for instance when he insisted that criticism of the Massachusetts delegation was

[1] Adams, "Works," IX, 342–346.

[2] Ibid., 338–340.

[3] "Familiar Letters of John and Abigail Adams," 5.

"envy at bottom," due to a kind of inferiority complex on the part of delegates from other states.[1] On the other hand, New Yorkers and Carolinians complained that New Englanders did not know how to act like gentlemen. Yet such unamiable aspects of human behavior may be taken too seriously. They were after all natural by-products of an overworked assembly whose members inevitably got on each other's nerves. The delegates unquestionably got much from each other and from their new surroundings.[2] What they learned in the sphere of politics is comparatively familiar; other aspects of their education have had less attention.

Even in committees, conversation was not always severely practical. In the Naval Committee, Adams tells us, the conversation of Governor Hopkins of Rhode Island took a wide range and "seemed to bring to recollection in all of us all that we had ever read." [3] A letter written by the Virginia scholar in politics, George Wythe, to John Adams, then abroad on a diplomatic mission, recalls similar intercourse with an "old friend whose company was entertaining and improving." Incidentally this letter from one congressman to another includes a three-line Greek quotation.[4] One New Englander, at least, acquired in such intercourse increased respect for social amenities. Adams observed that his countrymen, with "solid abilities and real virtues," were nevertheless deficient, not only in "the exterior and superficial accomplishments of gentlemen," but also in a proper "knowledge of the world." "Awkward and bashful" at times, they were yet on occasion "pert, ostentatious, and vain."

New Englanders profited also by the educational tours already mentioned, learning, for example, something about rural economy. Adams remarked on the excellent Maryland farms along the road between Philadelphia and Baltimore.[5] He was impressed also by the Moravians of Bethlehem—their

[1] "Works," IX, 459–461.

[2] Burnett, "Letters of Members of the Continental Congress," passim.

[3] "Works," III, 12.

[4] Ibid., III, 384.

[5] "Familiar Letters," 238.

agriculture, their "mechanical arts," the best inn he ever saw, and above all their freedom from bigotry. He was even moved to salutary discontent with New England husbandry in contrast with what he saw in Pennsylvania: "I am ashamed of our farmers, they are a lazy ignorant set." [1]

Education by travel was useful; but it was probably Philadelphia itself which Adams found most instructive. Though still a loyal Bostonian, confident of Boston superiority in most respects, he commended Philadelphia's market and her "charitable, public foundations." [2] Perhaps nothing impressed him more than the "scientific emulation" stimulated by the American Philosophical Society. Something like it should be provided in his home state: "A philosophical society shall be established in Boston; if I have wit and address to accomplish it." [3] In Paris, where he attended at least one session of the French Academy of Science, he observed that the Philosophical Society, with Franklin's personal prestige, had given Philadelphia a standing among scientists not accorded to the New England capital.[4] Such reflections presently bore fruit in a memorable clause in the Massachusetts Constitution of 1780, followed in 1781 by legislative incorporation of the American Academy of Arts and Sciences.[5]

The educational advantages of civilian service were paralleled by those offered in the Continental Army—the latter by no means confined to the military art. From a social point of view, many a young officer found in his army career some features of a finishing school. We are told that during a lull in the New York campaigns, a group of New Englanders profited by the instruction of John Trotter, a New York dancing master of high repute. Under his tuition they hoped "in due time" to "figure in a ballroom." [6] Fortunate, indeed, was the young Massachusetts surgeon who at one of Washing-

[1] Ibid., 240–242, 278.

[2] "Works," II, 395.

[3] "Familiar Letters," 207.

[4] "Works," III, 147; IV, 259–261.

[5] "Memoirs," I, Preface.

[6] Thacher, "Journal," 122.

ton's dinners found himself seated at the General's table, graced just then by the presence of Mrs. Washington.[1]

To take only a few of many possible illustrations, let us turn next to the medical profession. The significance of the Revolution, as of other great wars, for this branch of applied science, is a topic on which the layman must speak with caution; but we may at least suggest its importance for the social historian. It is not easy to exaggerate the value for young physicians, with meagre training and parochial experience, of the larger field opened up by the army hospitals. Consider, for instance, the case of John Warren, who went from a short apprenticeship with his brother Joseph to begin his practice at Salem, but whose energy and intelligent use of his new opportunities brought him in the closing years of the war to the direction of the army hospital in Boston. Here, we are told, he could command subjects for dissection without exciting alarm.[2]

Most important of all, perhaps, were the personal contacts thus established. In this way conceptions of medical science and medical practice, hitherto confined to a few centres, were much more widely diffused. Some of the more competent Americans doubtless learned from their colleagues in the French service, as Warren did when he pursued his medical studies with the help of books borrowed from the French officers in Boston.[3] As another example of French contributions, we may note that when, in the early nineteenth century, John Collins Warren investigated the Boston water supply, he went back to earlier studies of that subject by Surgeon-Major Féron of the French Navy, a friend of the older Warren.[4] There were some contacts also with the British and German services, especially after Burgoyne's surrender; Thacher's *Journal* of the war records the "skill and dexterity"

[1] Thacher, "Journal," 160.

[2] Harrington, "Harvard Medical School," I, Chap. III and passim; Mass. Hist. Soc., *Proceedings*, VII, 181 ff.; Batchelder, "Bits of Harvard History," Chap. V; E. Warren, "Life of John Warren," passim.

[3] Harrington, "History of the Harvard Medical School," I, 87. Cf. E. Warren, "John Warren," 190–191.

[4] E. Warren, "John Collins Warren," I, 70.

of the English surgeons.[1] Benjamin Rush reports, in the early *Transactions* of the American Philosophical Society, observations on tetanus which came to him from French officers in the Yorktown campaign, not directly but by way of the German physician, Schoepf.[2]

Still more significant, perhaps, was the effect of the war in extending the influence of a few outstanding leaders in the American faculty. Fortunately, the Revolutionary government was well situated from the point of view of expert medical advice. Some good work was done elsewhere; but Philadelphia, with its substantial group of European-trained physicians, with hospital facilities not to be found in the other colonies, and a young but vigorous medical school, could fairly claim the lead in medical progress. Boston, which had to meet the first shock of the war, was far less fortunate. It is significant also that, after one unlucky choice, the Continental Congress selected as the head of their medical service an outstanding leader of the profession in the person of John Morgan.[3]

For the student of the intellectual history of eighteenth-century America few documents are more interesting than the *Discourse* delivered by Morgan in 1765, on his appointment to the first American professorship of the theory and practice of medicine. In that address he outlined his views of medical education and practice; and also in strikingly modern fashion stressed the importance of scientific research.[4] With the highly controversial discussion of Morgan's administrative record and the events which led to his being superseded by his Philadelphia colleague, Shippen, we are not concerned here. The important thing is that a large number of army surgeons, drawn from all parts of the country, were brought in some degree under the influence of professional standards to which most of them were not accustomed. Many, no doubt,

[1] "Journal," 112.

[2] *Transactions*, II, 225–231.

[3] Cf. F. R. Packard, "History of Medicine in the United States," passim; Harrington, "Harvard Medical School," I, 38 ff.; Batchelder, "Bits of Harvard History," Chap. V.

[4] "Discourse upon the Institution of Medical Schools in America" (1765).

were incapable of learning much; but a fair number were trained for future leadership.

Some of the space commonly devoted by conventional historians to minor military episodes might well be spared to record these phases of educational progress. No doubt, the youthful James Thacher of Massachusetts was being educated when he sat one day at table "till evening, enjoying the conversation of the learned Dr. Shippen." [1] In a larger way, young John Warren was brought into relation with Morgan at Cambridge and later in New York. Having made effective use of his advantages in the military service, he was presently in a position to act with the Harvard authorities in the founding of their new medical department. The Harvard Medical School was thus, in some sense, a child of the American Revolution.[2]

The war undoubtedly stimulated interest in sanitation among many people, including the politicians. Samuel Adams, for example, commented on Congressional regulations "for keeping our Soldiers cleanly in their Encampments whereby their Health...will be preserved." Army chaplains were expected to coöperate by inculcating this virtue, along with godliness.[3] Dietetics also came in for Congressional discussion, with the discovery that something might be learned from British practice. Speaking of the importance of vegetables and vinegar for the army, Elbridge Gerry noted British superiority in this respect and added: "I fear We shall never have a Healthy and Vigorous Army without them." [4] John Adams agreed with Gerry and added his own critique of Yankee cooking: "Our frying-pans and gridirons slay more than the sword." [5] Of interest also are Rush's well-known efforts to substitute malt liquors for stronger beverages.

Several early papers of the American Philosophical Society

[1] Thacher, "Journal," 195.

[2] Harrington, "Harvard Medical School," I, 68–87; Ephraim Eliot in Mass. Hist. Soc. *Proceedings*, VII, 181 ff.; Batchelder, "Bits of Harvard History," Chap. V; E. Warren, "John Warren," Chaps. XII, XIII. In planning the requirements for the new medical degrees, Warren consulted Morgan's junior colleague, Rush.

[3] Burnett, "Letters of Members of the Continental Congress," II, 339, 376.

[4] Ibid., 191.

[5] "Familiar Letters of John and Abigail Adams," 259.

and of the American Academy at Boston reflect wartime associations and interests. The *Transactions* of the former included Rush's papers on fevers and tetanus, illustrated from the experience of the fighting services—American, French, and British.[1] Early *Memoirs* of the Academy included an army surgeon's account of his success in treating a gunshot wound without an operation, with this moral: "That surgeons may be too officious as well as too tardy." Unless very sure of their ground "they had better leave even the most desperate disorders to the management of nature, ever provident and generally adequate."[2] The experience of another medical officer was drawn upon for a paper on ventilation; and a third essay from a New Hampshire doctor stationed in New York described the curative values of the Saratoga springs. Féron's essays on Boston water supplies appeared in the same publication.[3]

In the second volume of the Philosophical Society's *Transactions* there was another contribution from a French officer —this time, however, in another branch of science and not from a medical man. M. de Grauchain, major general of the French Squadron, communicated from Newport in 1780 his records of a solar and a lunar eclipse. Astronomy had been his favorite avocation; and he could not forget his scientific interests even in the midst of war. "Eclipses," he wrote, "are the basis of chronology, and these eclipses in particular may one day serve to fix the epoch of American independence, one of the most interesting in the history of the human race."[4]

Some scientifically minded Americans shared this Frenchman's feeling that the time and place in which they lived gave a special interest to their work. In 1786, Benjamin Waterhouse, Warren's first colleague in the Harvard medical faculty, published his "Synopsis of a Course of Lectures,"[5] which suggests that he had such relations in mind. One topic was:

[1] *Transactions*, II, 210, 225–231.

[2] American Academy of Arts and Sciences, Memoirs, I, 544–545.

[3] Ibid., I, 542 ff., 554–556; II, Pt. I, 43, 170.

[4] *Transactions*, II, 239–246. Cf. American Academy, *Memoirs*, I, 151.

[5] Boston, 1786.

"What form of government and at what period most favorable to learning." "The arts and sciences," he thought, "commonly flourish immediately after civil wars and commotions." Evidently the members of the American Academy believed that the political revolution was an auspicious event for the scientists. The preface to the first volume of their *Memoirs* speaks of "that freedom which is propitious to the diffusion of knowledge, which expands the mind, and engages it to noble and generous pursuits." Americans were happy in "a stimulus to enterprise which few inhabitants of other countries can feel." The motto of the Academy was *Sub libertate florent*, conveying, as officially interpreted, "the general idea that arts and sciences flourish best in free states."[1]

In Morgan's utterances also there is something of this self-conscious Americanism carried over into the field of science. It appears even in his *Discourse* of 1765, but it was stimulated by the achievement of independence. In a paper read to the Philosophical Society after the war, Morgan recurred to the theme of his earlier address in a plea for American contributions to anatomy, now more necessary because some of the older ties had been broken: "Upon our own exertions must we, therefore, chiefly depend for building up the medical fabric, erecting useful temples of the healing art and diffusing the lights we kindle through this new world."[2]

In 1789, a few weeks before the inauguration of the new Federal government, one of the councillors of this Society addressed his fellow-members on American responsibility for scientific progress. In his "Essay on those inquiries in Natural Philosophy, which are most useful to the United States of North America," Dr. Nicholas Collin declared that science, though international in scope and in its ultimate objects, should take into account also national situations and interests. He outlined, therefore, certain inquiries which he thought especially suitable for Americans. I think I cannot more appropriately close the present paper than by quoting a few lines

[1] *Memoirs*, I (1785), Preface, esp. xi, xix.

[2] *Transactions*, II, 366 ff.

from Collin's essay read to your predecessors at their April meeting one hundred and forty years ago:

"Philosophers are citizens of the world; the fruits of their labours are freely distributed among all nations; what they sow is reaped by the antipodes, and blooms through future generations. It is, however, their duty to cultivate with particular attention those parts of science which are most beneficial to that country in which Providence has appointed their earthly stations. Patriotic affections are in this, as in other instances, conducive to the general happiness of mankind, because we have the best means of investigating those subjects which are most interesting to us." [1]

[1] *Transactions*, III, iii–xxvii.

THE TOMB OF THE MOGHUL EMPEROR BĀBUR IN AFGHANISTAN

BY A. V. WILLIAMS JACKSON

(Read April 18, 1929)

In the spring of 1926, while on a seventh journey to the Orient for purposes of scholarly research, it was my privilege to realize the dream of a quarter of a century by making a brief tour in 'long-forbidden' Afghanistan. My wife accompanied me, and special permission was granted to enter the mountain-barriered country, which we did by way of the Khaibar Pass from northern India. A Nash motor car formed the means of transport, threading its way over the high and dangerous passes, amidst the hopeless crowds of caravans of camels, donkeys, nomads, and Afghan traders that thronged the narrow ways with their bales of merchandise, household goods and chattels, guarded by ferocious dogs, and accompanied by their wives, children, chickens and tiny lambs, piled high on the humps of the drooling, snarling camels.

Accommodations on the journey, except when we were guests of the government, were crude, but the hospitality of the natives was always kindly to the strangers within their gates, even though they might at the time be nursing some inveterate blood-feud with the neighboring mountain clans. We visited Jalālābād, Kābul, Paghmān, but more particularly the out-of-the-way city of Ghaznī, famous nearly a thousand years ago as the royal seat of the mighty conqueror Maḥmūd of Ghaznī, 'Allah-breathing Lord,' and because the great Persian epic poet Firdausī spent thirty years at his court, when writing the Shāhnāmah, or 'Book of Kings.' So rarely is Ghaznī visited by Occidentals that Mrs. Jackson was stated to have been the first lady from the West to come thither.

Kābul, the capital, had a special historic interest for me since it was mentioned, long before the Christian era, in the Avesta of Zoroaster under the name Vaēkereta, which perhaps means the city or district 'divided into two parts' by the Kābul River. The old town showed many features that date back ages ago. By the side of it there is now being built a new city, planned on wholly modern lines. But my first object, on the day after our arrival, was to visit the tomb of Bābur (1483–1530), the founder of the Moghul, or more correctly Mughal, dynasty of India, popularly known as the Moguls.

Bābur, soldier of fortune, king errant, emperor, as we know him from his inimitable autobiographic memoirs, is one of the most fascinating figures in Oriental history. From Farghāna, his native province in Central Asia, he entered upon his martial career in the north, winning and losing cities by turns, but never swerving from his ambitious aims as a conqueror, which brought Afghanistan under his sway by force of arms and finally seated him on the throne of Delhi in 1526. He thus formed a connecting link between Central Asia and Hindustan, and laid the corner stone of the Moghul Empire.

Bābur died at Agra, December 26, 1530, in the forty-eighth year of his eventful life. His body was first interred in the Nūrafshān Garden on the left bank of the river Jumna, but the remains were later conveyed to his beloved Kābul, in accordance with the directions in his will, and were buried on the terraced hillside slope of the Shēr-darwāza, which is generally known as Bāgh-i Bābur, or 'Bābur's Garden,' about a mile to the southwest of the city.[1] No more delightful site than this could have been chosen, with its view overlooking the expanding plain that is watered by the river and enclosed by distant mountain heights. Here, in the earlier days of his kingship, Bābur may occasionally have joined in social revel with his friends; his own Memoirs show that,

[1] Consult also the footnote in the translation of Bābur's Memoirs by J. Leyden and W. Erskine, annotated and revised by Sir Lucas King, *Memoirs of Bābur*, 2. 426, n. 1, London, 1921.

though a Muhammadan, he was not averse at times to the wine cup.[1] Today his burial place is looked upon as a shrine, and pious Muhammadans make it a goal for pilgrim journeys, and have done so for four hundred years. Nor need we wonder that holiday folk foregather nearby, as was the case when we visited it.

It happened that this was the day of the annual county fair at Kābul. The country people flocked hither from miles around with their prize livestock decked gaily with ribbons, garlands and gilded horns, their choice horses groomed to a gloss and adorned with blue beads to ward off the evil eye, while champion young athletes stood ready to engage in the contests of wrestling and feats of strength. The Amir himself, King Amanullah, who later abdicated his throne in November 1928, but has since been struggling in vain to regain it, honored the festive gathering by his presence. I was so occupied in studying Bābur's tomb and its surroundings, and in photographing the inscription on the white marble slab at the head of the grave, that I completely missed catching even a glimpse of the Amir!

At the moment I had with me only my diary, since traveling with books is not easy in Afghanistan; but on returning to library facilities in America I found that several descriptions of the site had been given by others who had visited the tomb within the past hundred years, or had studied the contents of the inscription and other incised tablets by means of transcribed copies, or later even through photographs taken of the slab itself. A list of such references is recorded in the footnote below.[2] I shall, therefore, confine

[1] Bābur drank no wine till he was about thirty, but later he freely indulged in it in the company of boon companions, only renouncing it occasionally. There are many allusions to the subject in his Memoirs. See, for example, Beveridge, *The Bābur-nāma in English*, 1. 299, 302, n. 4, 371, n. 3, 386, and especially 387–388, also (for occasions of renouncing) 2. 551–552, 648. *Cf.* Leyden-Erskine-King, *op. cit.*, 2. 11–12, 88, 103, 280, 374.

[2] Bibliography. For the nineteenth century see Lt. Col. (later Sir) Alexander Burnes, *Travels into Bokhara*, 1. 141–142, London, 1834; 2d ed., 2. 121–122, London, 1835. H. G. Raverty, *Notes on Afghanistan*, p. 67, is referred to in Leyden-Erskine-King, *Memoirs of Bābur*, 2. 426, n. 2, as having translated the epitaph, but the volume is not accessible to me. There is a valuable short article on this and the

myself largely to the memorandums I made on the spot, but shall supplement them occasionally from other sources.

The garden-cemetery, oblong in shape and several hundred yards in length, lies on the next to the highest terrace. The precinct is enclosed on three sides by a white wall, into which are sunk arched niches to set it off; the side to the left as one enters has simply a low coping, which tops the stone breastwork parapet beneath and does not obstruct the delightful view. A clear stream from the heights above waters with its rivulets the formal beds of flowers which Bābur loved, as his Memoirs show; but no flowers were growing when we were there, because spring had not yet set in at Kābul. On the terrace just below, and near to the low-coped breastwork of stone that has been mentioned, there stands a small but beautiful mosque. It was built at large cost, in the year 1646 (A.H. 1056), by Bābur's great-great-grandson Shāh Jahān, after his conquest of Balkh and Badakhshān in the north. This we know from the inscription on it, as translated by Darmesteter.[1]

various inscriptions on tablets in the precinct, with texts and translations, by the French scholar James Darmesteter, in *Journal Asiatique*, Paris, 1888 (8me série, tome 11, p. 491–503). Darmesteter, though he did not visit Afghanistan, obtained these copies from the British Colonel Pratt, of Abbotabad in northern India, who had had them made by the *munshī*, or native interpreter, of his regiment during the English occupation of Kābul in 1879. Frequent references to this article are made below. For the twentieth century, special mention should be made of an article which became available to me only after the present sketch was almost completed; it is found in Sir H. H. Hayden's 'Notes on some monuments in Afghanistan,' in *Memoirs of the Society of Bengal*, Vol. 2, for 1907–1910, p. 341–346, Calcutta, 1911. Three of its pages (p. 344–346) deal briefly with Bābur's tomb, but are important because they include a translation which Sir E. Denison Ross made from the small but decipherable photograph of it as contained in Hayden's large plate of the whole precinct (Plate XVII). A reference to this could fortunately be added. Consult furthermore some data in the notable work by Mrs. Annette S. Beveridge, *The Bābur-nāma in English . . . translated from the original Turki text* (London, 1921–1922), Vol. 2, 710–711, and Appendices, p. LXXX–LXXXI; *cf.* also the picture of Bābur's grave (Frontispiece) although the inscription in this instance is undecipherable. Besides using my own photographic reproduction of the epitaph as a basis for the translation, I have also made use of the valuable photographs found in two other books. One of these occurs in the work of Oskar von Niedermeyer, *Afganistan* (bearbeitet von Oskar Niedermeyer und Ernst Diez), plate 37, Leipzig, 1924; the other photograph is by the American traveler, Lowell Thomas, *Beyond Khyber Pass*, p. 165, New York, 1925. Neither of these books, however, gives any translation of the inscription. For old sketches of Bābur's grave and surroundings, made by Atkinson nearly a century ago, see S. M. Edwardes, *Bābur: Diarist and Despot*, p. 130, *cf.* p. 119, London, 1926.

[1] See Darmesteter, *op. cit.*, p. 499–501; he used the transcript made by a munshī, as noted just above.

Within the cemetery precinct there are, besides Bābur's grave, those of four of his descendants, likewise with epitaphs.[1] One is the grave of his son Mīrzā (Prince) Hindāl; another, of a grandson Mīrzā Muḥammad Ḥakīm, who was a son of Bābur's successor, Humāyūn, and brother of Akbar; the third, that of a granddaughter of Bābur, named Raqīah (Ruqiyyah), daughter of Hindāl; and the fourth, that of a much later female descendant of Bābur, a daughter of Ālamgīr II, emperor in the middle of the eighteenth century and among the last of the Moghul rulers. This latter epitaph was the one I particularly noted because of the name of the princess, which was Gauhar Nisā Bēgam, 'Princess Pearl of Women.' In deciphering this and taking notes to work up later, I was aided by a young Afghan officer who had studied engineering in America for five years and was then employed in the Amir's service. The death of the princess, as I was able to determine after my return home, occurred in 1788 (A.H. 1202).[2] But all this is rather a digression; our interest centers in the grave of Bābur himself.

Bābur's grave, located more toward the farther end than middle of the precinct, as we pass through the portal, is of the simplest dignity, open to the sky under which he loved to live. My photograph shows a temporary wooden rail around it and a roofing of wood above (with an electric light suspended!), but this structure is intended only as a protection from the winter storms, and is removed in the spring. That is proved also from photographs taken by others in this century, as referred to in my bibliographical note above (p. 198, middle and end).[3] The grave-covering, which rests on a marble

[1] Darmesteter, p. 496–501. It is worth while to remark that the inscription on the tombstone of Mīrzā Hindāl shows that it was erected by the Moghul Emperor Jahāngīr upon the occasion of his visit in A.H. 1016 (the spring of 1607); furthermore, the inscription on Mīrzā Ḥakīm's slab records that it was set up in the same year by order of Jahāngīr.

[2] See *op. cit.*, p. 501–502.

[3] In this connection it would seem well to raise a new question for consideration, namely, as to whether the immediate aspects of Bābur's grave have undergone changes in the past four centuries. The matter involves that of sketches made nearly a century ago, and that of recent photographs taken within the past two decades. The sketch which was artistically drawn by James Atkinson before the middle of the

pavement, consists of three low double layers of white marble blocks, the bottom course being the larger, the two upper ones proportionately smaller. It is striking for its simplicity and chasteness, but my photograph fails to bring this out. At the head of the grave, but some inches from it, there is an upright slab of white marble with a beautifully inscribed epitaph, which is translated below. Some feet behind this there rises a large open-work screen, made of mortar and whitewashed; this is a *chirāghdān*, or 'lamp-holder,' with interstices in which to set dedicatory lights, but it is now undergoing considerable disintegration through the elements. From earlier accounts in the last century we have reason to believe that there was another slab, a footstone I should judge, corresponding to that at the head of the grave; if so, it has long since disappeared.[1]

nineteenth century (reproduced in Beveridge, 1. 367, and by Edwardes, p. 130) would seem to indicate that in earlier days there was more of a tomb structure. I fancy that the screen element in the old picture printed in V. A. Smith, *The Oxford History of India*, 2 ed., p. 324, Oxford, 1923, may have been from a drawing later by Daniel (not available to me, but referred to by Mrs. Beveridge, as quoted below in footnote 1). No trace of such an elaborate structure, however, appears in any of the photographs already referred to, nor in a photograph reproduced in a volume of the series which I compiled and edited on the *History of India*, 5. 261, New York, 1907. All the photographs show only the inscribed slab at the grave's head and the large lantern screen behind it. To me it would seem that Atkinson's graceful drawing, which was made from the hill above, may have foreshortened the two low turrets that capped the domed structure visible behind, outside the cemetery wall, together with the high walls outside to the rear of the enclosure, and may also have exaggerated the large lamp screen back of the epitaph slab. In addition, the screen sketch reproduced in V. A. Smith's *History*, p. 320, might be due to a similar exaggeration. My wife, who was with me, fully recognized this discrepancy between the sketches and the photographs when the various pictures were shown her later for comparison. On the whole, after mature deliberation, I am inclined to believe that, beyond possible ground-levellings, the grave of Bābur, with its immediate surroundings, has not changed materially since Jahāngīr's restitution or construction of the precinct, which shows his hand. Archæological research on the spot can alone settle the question.

[1] Sir Alexander Burnes, *Travels into Bokhara*, 2 ed. (1835), 2. 121, writes: 'The grave is marked by two slabs of white marble.' According to Mrs. Annette Beveridge, *The Bābur-nama in English*, 2. 710, this statement is further supported by the account given by Munshī Mohan Lāl, the scholarly interpreter who accompanied him, and likewise by the drawings of Daniel (not available to me). Her remarks on the subject are worth quoting: 'As is shown by the descriptions these two visitors (*i.e.* Burnes and Lāl) give, and by Daniel's drawings of the garden and the tomb, there were in their time two upright slabs, one behind the other, near the head of the grave. Mr. H. H. Hayden (see above, n. 3), who visited the garden in the first decade of the present century, shows, in the photograph of the grave, one stone only, the place of the one of the former two having been taken by a white-washed lamp holder (*chirāghdān*).'

An inscribed tablet over the entrance portal of the enclosure was set up by Bābur's great-grandson Jahāngīr, the Great Moghul, and records that he had made a pilgrimage to the tomb of his renowned ancestor and had placed this tablet to commemorate the visit.[1] It seems probable that he built or rebuilt the enclosing wall which his inscription marks. It is possible, though not stated, that he may have composed the epitaph for Bābur's headstone, since Jahāngīr himself was a writer and the author of his own Memoirs.[2]

We now come to the main point of our antiquarian examination—the epitaph so handsomely carved on the slab at the head of the grave of him who laid the foundations of the Moghul Empire in India. For aid in deciphering the somewhat intricately written five words of divine adjuration, which form the curving top of the inscription as my photograph shows, and for help in translating some less familiar terms in the epitaph itself, I take pleasure in thanking a pupil of mine, both in his undergraduate and graduate days, now University Extension Lecturer in my Department, Mr. Asadollah Beijan; he was always generously at hand when a problem arose. To him I also owe the accompanying handsome transcription

By way of comment on the matter I may add that, while inclined to believe there were formerly two slabs, I would differ from the above as to their position. To me it would seem that one stood at the head, the other (now missing) at the foot. Support for this view is given by the headstone and equally high footstone of the grave adjoining Bābur's (see the photograph in Beveridge, 2. 445, Frontispiece). I may further add that Atkinson's old drawing (as reproduced in Beveridge, 1. 367) also shows two stones, even though I have indicated above (n. 7) that his whole picture seems in certain respects open to criticism. For possible added support in favor of two slabs see below, n. 11.

[1] For the text and a translation of the inscription, which begins with an invocation, see Darmesteter, *op. cit.*, p. 495–496.

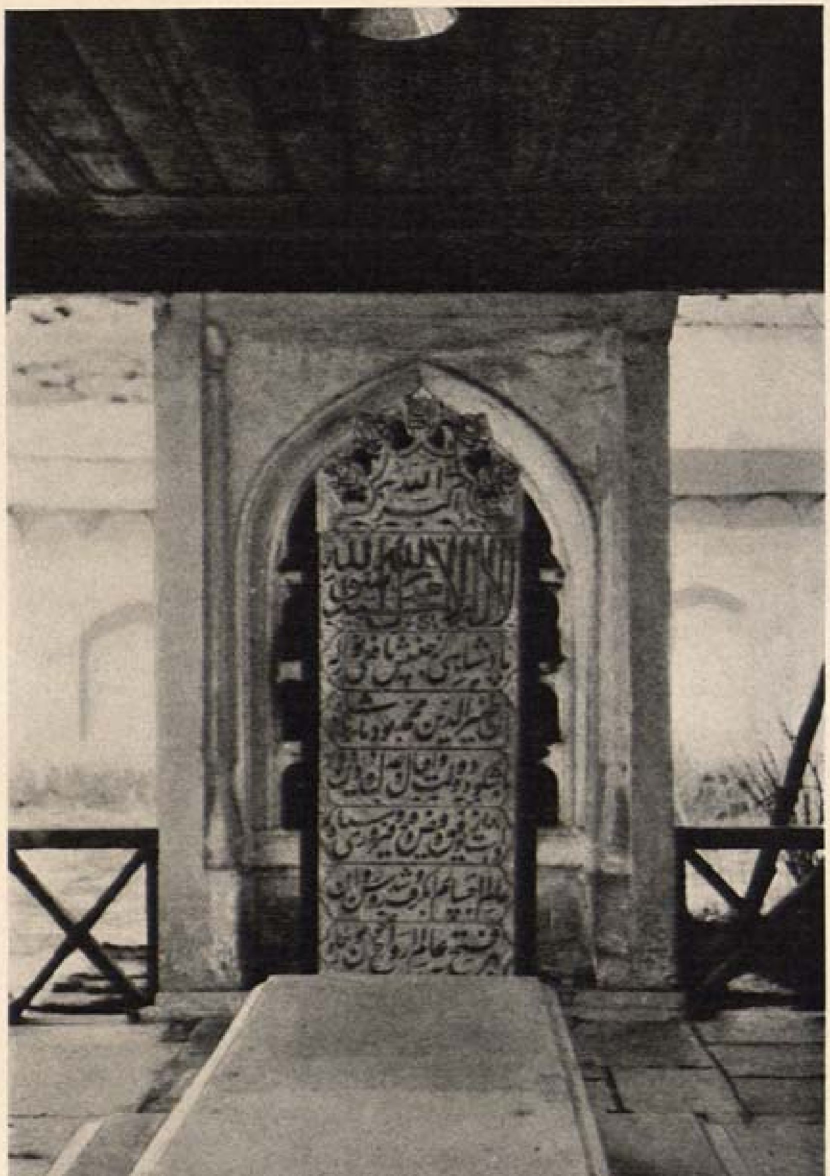
[2] As remarked above (n. 5), Jahāngīr was responsible for setting up the headstones at the graves of Hindāl and Ḥakīm, which are inscribed. Moreover, both the epitaph on Bābur's grave and the inscription by Jahāngīr on the mural tablet begin with divine adjurations, which, while different, are similar in pious tone. It is true that the epitaph is composed in verse while the tablet is in prose, but the choice of verse for the former would be natural, as lending special dignity to the lines inscribed over the dust of so famous a progenitor. We must add that in the account of his journey to Kābul in the spring of 1607, as recorded in his Memoirs, Jahāngīr makes no mention of his visit to the tomb, nor does he give any details about the city, his diary entries being largely confined to events connected with the journey. This, on the other hand, makes the inscriptions of Jahāngīr in the cemetery even more valuable from the historic standpoint.

یا سبحان یا ملک یا فتاح یا عادل یا قدوس

الله اکبر

لا اله الا الله محمد رسول الله

پادشاهی کز جبینش تافتی نور اله
آن ظهیر الدین محمد بود بابر پادشاه
با شکوه و دولت و اقبال و عدل و دین و داد
داشت از توفیق و فیض و فتح و فیروزی سپاه
عالم اجسام را بگرفت و شد روشن روان
بهر فتح عالم ارواح چون نور پگاه
شد چو فردوس مکان رضوان ز من تاریخ جست
گفتمش فردوس دایم جای بابر پادشاه



into current Persian letters, Persia having ever been famed for its calligraphy.

I now give here a line-for-line rendering in English, without attempting to represent the rhymed verse of the original. The various notes of reference will show that careful attention has been paid to previous translations, which are recorded in the bibliographical note above (p. 198).

TRANSLATION OF THE EPITAPH ON BĀBUR'S GRAVESTONE

(i) O Worthy of Worship, O King, O Victorious, O Just, O Holiest One!
(ii) God is Supreme.
(iii) THERE IS NO OTHER GOD BUT GOD (ALLĀH);
(iv) MUḤAMMAD IS THE APOSTLE OF GOD.
(1) An Emperor (*Pād āhī*) from whose brow shone the Light of God
(2) Was that [1] Ẓahīru'd-Dīn Muḥammad Bābur [2] Pādshāh.
(3) Together with majesty, dominion, good fortune, justice, religion and law,
(4) He had an army (possessed) of power, grace, victory and triumph.[3]

[1] Mrs. Beveridge (*op. cit.*, 2. 711, n. 1) plausibly suggests that the demonstrative pronoun (*ān*, 'that') may here refer back to an original inscription on the second, but now missing, upright slab, which presumably would contain Bābur's name. If correct, this would lend added support to the view presented above (p. 200, n. 1), that there were two stones and that Jahāngir (see n. 10) may have caused both to be erected and inscribed.

[2] The descriptive attribute in Bābur's name Ẓahīru'd-Dīn signifies literally 'Support (backing) of the Religion,' *i.e.* 'Pillar or Defender of the Faith.' *Cf.*, similarly, Beveridge, *op. cit.*, 2. 711, 'Back-bone'; ditto, Edwardes, *Bābur*, p. 131. Regarding the correct pronunciation of the name Bābur (not Bābar) see the article by Sir E. Denison Ross in *Journ. Asiat. Soc. of Bengal*, new series, Vol. 6 (1910), p. iv–vi (printed near the end of that volume as an 'Introduction' to his memoir on the 'Divan-i-Babur Padshah' at the beginning). *Cf.* (with approval) E. G. Browne, *History of Persian Literature under Tartar Dominion*, p. 184, n. 2, Cambridge, 1920. The name is not fully vocalized in this inscription.

[3] The meaning of this stanza seems to be that, in addition to all kingly qualities, Bābur had a good army to support him. Darmesteter (p. 494–5) renders it somewhat differently: 'Par son succès, sa grâce, sa fortune victorieuse, il tenait son peuple dans la majesté de la puissance, de la justice et de la loi.' Ross (in Hayden, *op. cit.*, p. 346) translates, 'With splendour, wealth, good fortune, justice, probity and faith, He commanded a force composed of Divine Bounty, Grace, Victory, and Triumph.' Beveridge (p. 711) gives somewhat more freely, 'Together with majesty, dominion, fortune, rectitude, the open-hand (!) and the firm Faith, he had share in prosperity, abundance and the triumph of victorious arms.' Perhaps this version may have been influenced by the earlier one by Raverty, which is unavailable to me, as already stated.

(5) He conquered the (material) world of bodies and became of illumined soul.[1]
(6) For (winning) the victory of the World of Spirits,[2] like the light of dawn.[3]
(7) When Paradise became his abode, Riẓwān[4] asked of me the date;
(8) I answered him (in a chronogram): 'Paradise is forever the abode of Bābur the Emperor.'[5]

[1] Beveridge (p. 714) renders *nūr-i rawān* (or *ravān*), etc., somewhat differently: 'became a moving light; for his every conquest he looked, as for Light, towards the world of Souls'; but Darmesteter (p. 495) and Ross (in Hayden, p. 346) have practically the same rendering as I have given.

[2] That is, after conquering the world Bābur triumphed finally by gaining Heaven.

[3] The photographs prove that *pagāh*, 'dawn,' is the correct reading, thus supporting the conjecture made by Darmesteter (*op. cit.*, p. 494, n. 2) to emend the munshī's transcript which gave *bakā*.

[4] Riẓwān is the door-keeper of Paradise. Darmesteter (p. 494, n. 3) noted that this word was missing in the munshī's transcript but could be supplied from the rendering 'Roozvan' in the quotation of this stanza by Burnes, *Travels* (2 ed.), 2. 121, and is required by the metre as well as the sense. The correctness of this conjecture is now fully supported by photographic evidence, see next note.

[5] My photograph, taken from the front, lacks these last two lines, but I have supplied them from the text which Darmesteter reproduced from the munshī's transcript. This has been further compared with the partly legible, or incomplete, last two lines in the photograph by Niedermeyer and that by Lowell Thomas (see above, p. 198 n.). I have compared them likewise with the printed text of Ross (in Hayden, *op. cit.*, p. 345) which became accessible later, as noted above (p. 198 n.).

The concluding sentence, *Firdaus dā'im jāy-i Bābur Pādšāh*, 'Paradise is forever, etc.,' is a chronogram giving the date of Bābur's death. This was noted by H. G. Raverty, according to Leyden-Erskine-King, *Memoirs of Bābur* (revised edition), 2. 426, n. 2, London, 1921, but is not available to me. Darmesteter, *Journal Asiatique*, 1888, p. 495, n. 1, worked out the chronogram with the following results, with which my pupil Beijan's independent computation wholly agrees:

Firdaus	= 350 (80 + 200 + 4 + 6 + 60)
dā'im	= 55 (4 + 1 + 10 + 40)
jāy	= 14 (3 + 1 + 10)
Bābur	= 205 (2 + 1 + 2 + 200)
Pādšāh	= 313 (2 + 1 + 4 + 300 + 1 + 5)
Total	937 A.H.

We know from references in Muhammadan historians that Bābur died in Agra on the sixth (or fifth) of Jumāda I, 937, of the Hijra era. This would correspond to December 26, 1530, the generally accepted date for Bābur's death. See, for example, Stanley Lane-Poole, *Mediæval India*, p. 317, New York, 1903; V. A. Smith, *The Oxford History of India*, 2 ed., p. 323, Oxford, 1923.

Regarding Paradise as the abode of Bābur we may add that the epithet *Firdaus-makānī*, 'Dweller in Paradise,' was a posthumous title given him; see A. S. Beveridge, *History of Humāyūn by Gul-Badan, translated*, p. 110 ff., London, 1902.

The visit to Bābur's tomb made a deep impression upon me during the entire stay at Kābul, and remains a vivid memory today. I could not help thinking of his marvelously eventful and diversified career, crowned with triumph—and with death. 'After life's fitful fever he sleeps well' might best express it. This was the minor chord that kept echoing through my being as the prelude to the journey next to be made, a journey to far-off Ghaznī, the once-famed capital of Maḥmūd the Ghaznavid, an Asiatic conqueror five centuries earlier than Bābur. But after all

'The path of glory leads but to the grave.'

THE CONCEPT OF NATURE IN PHILOSOPHY AND LITERATURE; A CONSIDERATION OF RECENT DISCUSSIONS

By ALBERT SCHINZ

(*Read April 18, 1929*)

THERE is no need to use much time to prove that the term 'nature' is one of the most perplexing in the vocabulary of science, art and literature.

Every one claims right to use it: to remain only in our Western civilizations, the Greeks and Latins used it constantly and claimed amongst other things that their social system of classes of masters and slaves was based on nature; the Christians came appealing to nature to proclaim equality of all men before God; the Renaissance appealed to it to repudiate the unnatural claims of the medieval asceticism; again the classics, Boileau, Racine, Fénelon in France, Shaftesbury, Dryden, Pope in England appealed to nature constantly, saying that reason and nature are one to command and discipline human passions. Of course the battle cry of Romanticism against Classicism was nature: let nature have its course![1] Now were to come the Realists, and in the name of what were they to drive out the Romanticists, if not in the name of *Nature*? . . . And the newcomers, foes of Realism and Naturalism (Impressionists, Futurists, Cubists) again succeed in bringing in the rights of nature.

The word 'nature' by itself would seem, then, to have really no particular meaning at all, for, having so many meanings is to have practically none. Or, at least, it has only a negative sense: everybody seems to use the word nature to refute some

[1] To add to the confusion the painter David, called upon to reform the wordly art of 18th century painters like Watteau, and join the romantic movement, re-introduces the Greeks and Romans on his canvasses and calls them "nature" at the very moment when the Hugo, Vigny, Musset tried in the name of nature to purge French literature from the Greeks and Romans of Corneille and Racine.

one else who had used the same word nature to define his position.[1] One may even be tempted to identify in philosophy the concept of nature with that of liberty. Everybody wants 'liberty'—although for different purposes, often for diametrically opposite purposes. In the early years of our era, the Christians claimed liberty against the Pagans, and when Christianity had won over Paganism then the Pagans—or freethinkers—claimed liberty against Christian philosophers; ever since the sixteenth century French Protestants cry for religious liberty against the Catholics; and in England the Catholics cry for liberty against the Protestants.

There is, however, a serious difference regarding the use of the two words *nature* and *liberty*. Something more is implied when the word nature is referred to: etymologically, of course, *natura* (from the Latin *nasci* to be born) means the thing at the outset, as it was when brought into existence. Then, when a philosopher or an artist calls upon nature, he means that he wants to start his investigation all over.

And as Truth is not easy to perceive and thus as, obviously, there will be often good reason—after having been led astray—to start again and to go back to the *natura* of things, this fully explains, even warrants, the ever-returning term of nature under various pens.

Even so, of course, nature is still a purely negative notion —although it may now, at any time develop into a positive concept—but this leads us to a new set of considerations. In

[1] Do not expect any help from dictionaries, which, of course, only *state* the various meanings, but cannot be expected to reconcile them, or even harmonize them. Webster offers nine fundamental meanings, each with *a*, *b*, *c*, . . . subordinate meanings. Murray offers forty-four meanings. In French, Littré offers twenty-nine meanings; and the *Philosophical Dictionary* of Lalande, in a long statement, and which is, moreover, very judicious, is obliged also to recognize endless "notions" for which the term "nature" has been called upon.

Neither will special books, like Lange's famous *History of Materialism*, or Stallo's *La matière et la Physique moderne*, or again Boutroux's *De la Contingence des lois de la Nature*—to give only some of the paramount authorities—bring to a satisfactory conclusion the "nature" puzzle.

Let us not fail to mention here the vigorous effort made by Professor Lovejoy, of Johns Hopkins University, who has studied for several years with his students, the history of the concept "nature." He has compiled a very interesting list of sixty-four different meanings, all borrowed from trained philosophers in the course of centuries.

the eyes of the scholar and philosopher, any phenomenon represents only the result of previous existing conditions or *phenomena*, viz. the effect of a cause more or less complex. In other words one *natura* is the outcome of a previous *natura* which explains it, and, indeed, what we want to do is to get back to the original *natura naturans*, mother of all subsequent *naturæ naturatæ*. This is the very thing which the first philosophers boldly tried to do, viz. having had no experience as yet of the complexity of the task, they got at once to solve the problem of the universe, to the *natura naturans*, or as Taine called it, the *universal axiom* the key of all the secrets. From hurried generalizations they proceeded at once to philosophical **systems**: the origin of all things. The *natura naturans* was either fire, or water, or earth (matter) or air (gas) . . . these *naturæ* which even to-day the popular language keeps calling elements.

It did not take so long to realize the futility of any of such individual systems; but it took long to give up the hope of settling thus, by a stroke of the pen, so to speak, the riddle of the universe and of nature. Systems followed upon systems, the tendency being to get beyond concrete or material *natura* or *physis*, and towards more after-nature or *metaphysical* principles: All those attempts at metaphysical systems can be summarized in the expression of one—the most famous—that of St. John: *In the beginning was the Word* . . . which is equal to "In the beginning was the Unknown."

After long, persistent and vain efforts, however, philosophers were driven to adopt a new method, slower but surer; they have recourse to-day to what we call division of labor, *i.e.* attacking the problem piecemeal; only ultimately hoping—perhaps—to reach general *natura* by combining the results obtained by investigations in various fields—the chemical *natura*, the physical, the cosmological, the physiological, etc., *naturæ*. These philosophers we call now rather *naturalists* or *scientists;* but until the eighteenth century, especially in English, the word philosophy by itself, connotated *natural philosophy* as distinguished from *metaphysical* or *theological* philoso-

phy which had prevailed up to the Renaissance.[1] To-day things have changed entirely, and when we call men philosophers we rather refer to such scholars as are not *natural* philosophers, *i.e.* investigators in *natural* sciences.

The "Philosophers" of to-day are really men concerned with some problems, not of truth in itself, but of a truth of *practical* bearing on some spheres of life; they are men who are on the diet of *Primo vivere, deinde philosophari*. And they are—to use a modern term—*pragmatic philosphers* all of them, although they do not often acknowledge it; they want to organize society, family, to bring up children, to organize religious life, to get at a philosophy of art—they are indifferent really to truth in itself, whenever truth does not seem to touch the interest of humankind, or whenever no relation as yet can be seen.

But these pragmatic philosophers (moralists, politicians, educators, preachers, artists, etc.) claim the word "nature" just as much—and, probably, *even more* than *naturalists;* they claim it in a different, and perhaps less justified, way.

What then do they mean by "nature"? They mean that if they cannot go back to nature in the cosmic or universal sense, they intend to go back some distance—namely to the point where reality is important for the practical problem in which they are interested. For instance, they will try to go back to the aspect of man's *natura* when he enters the state of organized society; or to the *natura* of man in the domain of religion, aside of, or previous to, what a revelation of some sort may have added to the *natura;* or again, they will try to describe the *natura* of man as he is able to experience esthetic pleasures. . . . But each one is indifferent to what a *natura* will be outside of his own province. In consequence we will be confronted with a political *natura* of men, a religious, an ethical, a psychological *natura*—and these *naturæ* may as well differ among themselves as not. Let us illustrate:

[1] In the name given to Franklin's 'Junto,' *American Philosophical Society for the promotion of useful knowledge*, undoubtedly that adjective "philosophical" meant what is conveyed to-day by the word "scientific"; it was to be the *American Society of Natural Sciences for the promotion of useful knowledge*.

Some one studying politics will find that his problem consists in getting the interests of all the members of the political body to harmonize, and this will mean principally checking the claims of individuals which threaten to disturb the social equilibrium: he, therefore, will consider that *natura* means egoistic dispositions in men; or, to use a familiar term, he will consider man as *not moral* by nature.

Another, a psychologist let us say, or a student of ethics will take no heed of the political problems and of the attitude of man towards the State, but he will insist upon the existence fundamentally in all human being of benevolent dispositions towards each other, sympathy for the suffering and for the weak and oppressed beings: he therefore will consider man a disinterested being, or to use a term corresponding to the one above, he will consider man as a *moral* being by nature.

Still another, a theologian, will take into consideration that the best souls among men (St. Paul, Augustine, Pascal and most christian—and even oriental—saints) have testified to a state of disquiet and torment within them: and the philosopher-theologian will infer that there is in humankind something fundamentally wrong, some mysterious evil in which man has in some way participated and for which he feels the necessity to atone; the philosopher-theologian will adhere to what is known as the doctrine of the original sin; therefore he will consider that man is by nature a *wicked being.*

Still another, also interested in the moral suffering of men on this planet, their longing for better things (he will be a poet like Eschylos, Lucretius, or Racine) will be convinced that man is a mere plaything in the hands of an irresistible destiny a victim of hateful passions with which he was born: therefore he will consider that since man cannot be held responsible for either bad or good, he is by nature an *amoral* being.

One can see that in these four cases of bad, good, wicked and amoral, we have perfectly acceptable uses of the term "nature"—for, in each case, the description rests on plausible sets of observations accepted by men whose mental superiority and sincerity cannot possibly be doubted, except by fana-

tics:—yet these four uses are obviously at odds with each other: how can nature be good if bad, or vice versa? how can it be disinterested if wicked? and finally how can it be termed good, bad or wicked if fatally determined to be what it is?

And the essence of human nature considered universally and which may reconcile these various uses (or finally disqualify one or more of them) cannot be given definitely before real *natural* philosophy—and not mere pragmatic philosophy—has accomplished its task. There is simply no hope, for the present at least, and thus when we speak of these *naturæ naturatæ* of pragmatic philosophers, we must realize that none can claim ultimate value.

But as man is not willing to wait to live, he will have to continue to let the philosopher build his political, religious, ethic, esthetic structures on precarious foundations.

Their relative strength depends on the accuracy of observation, and what can be done is to keep on with our present method. Surely observation can always be made more accurate and our notions improved in clearness. Of this the writer would like to give an illustration. The word nature has been perhaps more abundantly used by the pragmatic philosophers discussing romanticism in philosophy, art, and literature than by any others.

To get a clear view of the problem, the best is to go back to Rousseau's writings, Rousseau being considered the father of Romanticism. Let it be well understood however that Rousseau did not *introduce* the great battle about nature; when he came, that battle for or against nature was in full swing—this has been indisputably established by recent scholars—but we might say that Rousseau focussed the discussion; and ever since the eighteenth century, there is a sort of tacit understanding to consider him as having given the discussion its fundamental aspect.

It is our first duty, of course, to pay a well deserved tribute to former investigators who have studied that problem of the use of nature by Rousseau and Romanticism, such as

Hoeffding, Seillière, Masson, Ducros, Mornet and others. Their respective efforts certainly contributed to a better understanding of the various problems involved. At the same time it is our opinion that they failed to get to the heart of the problem, in as far as our point (which seems really the most important) is exactly the one that has not been adequately studied; namely the meaning of nature when applied strickly to the ethics of so-called romantic writers; for it is that which brought about the animated discussions between Rousseau and his contemporaries—theologians or Encyclopedists.

In the first place, we must recall the fact that Rousseau himself in the field of ethics, does not use the word nature in in a consistent way. Let us illustrate:

See first in his *Letter to d'Alembert on the Theater*—in which he so severely attacks what he considers the dangers of the theater for the people. There is one page specially where the word nature appears several times (pages 233–4, Vol. III, edition Hachette) but appears to designate in turn: (*a*) the natural inclination of men towards sensuality, (*b*) the *natural* inclination of men to condem sensuality, the *natural* sense of of shame. Thus: here Rousseau discussing the doctrines of philosophers who contend that sensuality is the obvious expression of human nature, writes: "Is it my affair to account for what nature has done? " [Est-ce à moi de rendre compte de ce que la nature a fait?] and incidentally in this case, of course, Rousseau recommends not to follow nature. But on the very same page, Rousseau speaks of that "ephemeral philosophy . . . which endeavors to suppress the outcry of nature [of shame and pudor] and the unaminous condemnation by humankind," and in this case, of course, Rousseau strongly recommends to follow the voice of nature.

Our second example is from the 5th book of *Emil*, the Treatise on education. Here Rousseau ignores entirely that *moral* "outcry of nature" which is the expression of "unanimous condemnation of mankind," and takes into consideration only the *primitive* "outcry of nature": but even then he is far from consistent, praising nature in one case, condemning

it in another. He discusses various gifts that nature bestows on women: one of them is beauty, another is cunning—"Cunning," he says, "is a natural gift to the sex, and feeling confident that all natural inclinations are good, I am of the opinion that this one be developed too; the only question is to prevent misuse . . . ," II, 342. Here then nature again is to be followed. But when it comes to beauty, Rousseau advises *not* to develop natural beauty, the reason being: "Unless a woman be an angel, her husband will be the most unfortunate human being, and if she were an angel, how can she prevent that he be surrounded by enemies," II, 343.

This is enough to show that to talk of *nature* in Rousseau and Romanticism without further specifications is impossible. —We say impossible: it *ought* to be impossible; alas we know only too well for having had to read a whole library about it that it IS possible.

Moreover, Rousseau himself was well aware of the different uses possible for men discussing ethical problems, of course, and he is not half as much to blame for having sometimes been loose in his vocabulary (for which he apologizes) as are the so-called scholars who did not try to make out from the context what he meant, before launching absurd attacks. Rousseau was aware of it, as illustrated for instance in his novel *La Nouvelle Héloïse:* There, we see the Baron d' Etanges invoking his authority of a father to refuse Julie as wife to a man like Saint-Preux not belonging to the upper social class. But this appeal to the natural rights of a father, Saint-Preux challenges by appealing to what *he* calls rights of nature, viz. right of love: "Et quand vous osez réclamer la nature, c'est vous seul qui bravez ses lois"—"and when you dare appeal to nature it is you who repudiate its laws" (Part III, Letter II).

A further proof for any careful reader that it ought to be perfectly clear that Rousseau was not unaware of the uncertain meaning of nature, is found in this passage from his *Third Dialogue* in which is discussed the book of his contemporary, Baron d'Holbach (*System of Nature*): "Our philosophers never fail to solemnly spread out that word *Nature* on

the front page of their writings. But open the book and you will see what metaphysical jargon they decorate with this name." And even better would be the following from *Emil* (Hachette, II, 284–5, *Profession de Foi du Vicaire Savoyard*) "Fuyez ceux qui, sous prétexte d'expliquer la nature, sèment dans les cœurs des hommes de désolantes doctrines. . . . Sous le hautain prétexte qu'eux seuls sont éclairés, vrais, de bonne foi, ils nous soumettent impérieusement à leurs décisions tranchantes, et prétendent nous donner pour les vrais principes des choses, les inintelligibles systèmes qu'ils ont bâtis dans leur imagination" (284–5).

Rousseau does more than that: he hands us himself the key to the misunderstanding (how it is that commentators have not made more use of this key, will be explained later); he gives it repeatedly, and, *e.g.*, when he says that he wants to discuss not "the state of nature of man" but "nature of man" (not "*L'état de nature de l'homme*," but "*la nature de l'homme*"); and when, in *Emil*, he speaks of the purpose of education being precisely: "**to form** the natural man" (*homme de la nature*, II, p. 226).[1]

Plainly this means that the word *nature* means a different thing when it applies to a certain early phase of human life, and when it applies to a later phase. There is the nature of man before he was "formed," and the nature of man after he was "formed." The nature of man, before he was "formed" deals with nature as it immediately appears to us in the primitive man or in man at his birth if, by abstraction, we imagine him free from what may be inherited traits.

But it so happens that in this *natura* of man in the state of nature there is an element which, in developing, is going to bring about such a change that a second state of man will be brought about, which will again be called *natural* state since it was brought about naturally by a natural development of the primitive nature. In other words there is a *virtual nature* contained in the *original nature*. This element

[1] Repeated when he speaks of the education of Sophie, the mate of Emil: "After having endeavored *to form* the natural man . . . let us see how must *be formed* also the wife that must be the mate of that man" (II, p. 334).

in the original nature is a power of understanding conditions of life, and seeing possibilities of changes, and ultimately a desire and a will to change one's own attitude towards reality; this element is intelligence or reason. A being with no intelligence—or with an amount of intelligence insufficient to suggest a desire for a change—will remain in the original state of nature. Thanks to his superior intelligence, man can undertake to direct his own destiny, to break away from the original passive nature, control his instinctive inclinations, his passions—and evidently the outcome of this all would be to realize a higher kind of life than the original *natura* can afford. It is of great importance to realize now that those two *naturæ* are not only different but opposed to each other:

(1) the undeveloped one *i.e.* the one that expresses ourselves *minus* intervention of reason, in which no active element plays a part, the *passive* nature of passions;
(2) and the developed one *i.e.* after intelligence has suggested to control the original *natura*, the one which takes a hand in subduing passions.

We have names for them, the *romantic nature* and the *moral nature*. To use the same word 'nature' to designate them both,—without, most of the time adjectives, to distinguish them—is bound to bring confusion.

It ought to be said that when Rousseau came, such ethical matters had just only begun to be discussed by independent thinkers (*i.e.* independent from theological philosophy), and it is not surprising that the effort to get to clear-cut notions was accompanied by groping in the philosophical vocabulary:—while the adjective *moral* was occasionally used in connection with nature, the adjectif romantic did not even exist, and none existed that would clearly convey this notion as opposed to the notion of *moral nature*. But that students of Rousseau to-day like Seillière, Lasserre, Lemaître, Bourget, and in America I. Babbitt, P. E. More should continue to take advantage of the confusing vocabulary to entirely misjudge, at times Romanticism, and always Rousseau, this is not acceptable.

Their contention is that Rousseau was an advocate of the *romantic natura*—which contention, in the writer's opinion, is not supported by Rousseau's writings. The bulk of proof has been given elsewhere (Schinz, *La Pensée de Rousseau*, Paris, 1929); we can only summarize briefly the argument.

It is especially in *Emil* that Rousseau discusses nature; this treatise on education is also the last constructive writing of Rousseau—hence the one where we can find his final views of the matter. Let us then turn to *Emil*, and pick a few passages referring to our problem.

On pages 182–3 (Vol. II, ed. Hachette of Works) discussing passions he plainly says that passions—or at any event strong passions which are fatal to his happiness—put man "hors de la nature." This obviously proves that he means to oppose what is for him a MORAL nature to a *romantic* nature.

On page 184 he discusses the strongest passion that may agitate man, *love*. Here are his words: "One sex is attracted by the other; here is the action (mouvement) of nature." . . . And at the end of the paragraph (fourteen lines further only), the same Rousseau writes: "Far from love coming from nature, love means the ruling and control of man's inclinations. . . ." Here again it is absolutely evident that Rousseau has in mind two kinds of love, love in the romantic sense (in which man simply yields to his senses) and love in the moral sense; and moreover that he decides against the former and for the second which he calls "véritable amour," viz. the love a man is only capable of experiencing after having used his intelligence, after having had "connaissance" of the matter, "after having thought matters over (jugé), after having compared (with the love in the romantic sense)." All this just illustrates the fundamental phrase, again in *Emil:* "One must not confuse what is nature in the primitive state of man, and what is nature in the civil state."

Again we read this: (Hachette, II, p. 148; Emil, III): "Tout homme veut être heureux, mais pour parvenir ă l'être il faudrait commencer par savoir ce que c'est que le bonheur. Le bonheur de l'homme naturel est aussi simple que sa vie; il

consiste ă ne pas souffrir: la santé, la liberté, le nécessaire le constituent. Le bonheur de l'homme moral est autre chose; mais ce n'est pas de celui-lă qu'il est ici question. . . ."

And further on page 257, this: "Nous croyons suivre l'impulsion de la nature et nous lui résistons; en écoutant ce qu'elle dit ă nos sens, nous méprisons ce qu'elle dit ă nos cœurs: l'être actif obéit, l'être passif commande. La conscience est la voix de l'âme [nature morale], les passions sont la voix du corps" [nature romantique].

Let us end by a passage (on page 249) in which Rousseau, in as many words, *opposes* the two natures—only he reverses what we call the chronological order, *i.e.* he mentions first—so as to give it a place of honor—the moral natura which, psychologically speaking, appears on the scene only second. "In meditating on the nature of man [the teacher tells his pupil Emil] I thought I discovered in it two distinct principles [of action], ONE of which takes him up to eternal truths, the love of [justice and] moral beauty, to regions of the intellectual world the contemplation of which delights the man of wisdom, AND the other which takes him back in his own base self, made him a slave to his senses, to passions . . . and went against all that the first inspired" (p. 249).

Is it not pertinent to ask how the men we have just mentioned have a right to stamp Rousseau a "romanticist" without just ignoring such passages as these?

We beg leave to quote now two passages where we find proof that Rousseau not only dissociates romantic nature and moral nature but expressly associates his *moral nature* with reason:

1. (P. 445.) Laws of men are laws of contigency, they are precarious, "but the eternal laws of nature and order stand. They are to the man of wisdom positive laws, they are written in the heart of man by reason [Note: Of course here, he means they are *virtually* written . . .] that is to say that man must become an obedient servant [to wisdom of reason] to be free [from his original passive self].

2. (P. 226.) "In endeavoring to form natural man, the idea

is not to make of him a savage . . .; but it is enough that, imprisoned as he is in the social whirlwind, he may . . . see things with his own eyes, that he feels with his own heart and *that no authority governs him except that of his own reason.*"

Our conclusion, then, after a reexamination to the text is that Rousseau is holding exactly these views which the men just quoted (also many others) are reproaching him for repudiating. When you go beyond the letter to the spirit, the association of Rousseau's name with Romanticism is wrong;—at least if you hold (as these men do) to the usual definition of Romanticism as a justification of man's claims to yield to the appeals of nature in the original sensual form, and which is described by Taine in these striking words: "What we call nature is this brood of secret impulses, often maleficent, generaly vulgar, always blind, which tremble and fret within us, ill-covered by the cloak of decency and reason, under which we try to disguise them; we think we lead them and they lead us; we think our actions are our own, they are theirs." (*Hist. de la litt. anglaise*, 9° éd. Vol. IV, p. 130.) If you oppose romanticism to the classic idea that man must control passion, emotion, sentiment, Rousseau is a classicist and not a romanticist. It is interesting to note here that this very Taine, most powerful repudiator of romantic individualism, is, however, the very man who pointed out a similarity between the classic ideal of moral discipline and the natural man of Rousseau, *i.e.* the normal man in whom will be found an equilibrium of the constituting romantic elements of man controled by reason or moral nature. (Quoted by Vial, *Doctrine de l'éduc. de Rousseau;* Delagrave, p. 66.) And this view of the matter is well justified, as can be seen by studying the heroine of Rousseau's novel, *Julie* who finds happiness in marrying not according to her romantic passion for St. Preux, but in marrying Wolmar the representative of sober reason—*i.e.* of reason that has developed our moral nature.[1] She is the sister

[1] Entirely in accord with Taine—and the views expressed here—is Alfred Fouillée the creator of the doctrine of "idées forces";—although he does not state the theory in connection with the specfic problem of the meaning of nature in Romanticism. Cf. *Idée moderne du droit*, V, 5, page 340.

of the Cornelian heroines, Chimène and Pauline—and not their romantic counterpart, as traditionally stated.

Our next problem is: how did the persistent ranking of Rousseau as a Romantic and as opposed to the ethics of discipline of human romantic nature come about? Various causes have contributed; several of a merely contingent nature; but two are of a more fundamental nature.

The first one is, that, very soon after the death of Rousseau (indeed one might even say while he was still alive—as can be seen for instance from the sort of success his books obtained with many contemporaries, especially ladies) a legendary Rousseau was substituted in the mind of the people for the actual Rousseau as he is reflected in his written works. And this again can be traced to the fact that the writings of Rousseau really fall in two classes:

(1) those in which he speaks a good deal about himself, and indisputably reveal a romantic nature (*Confessions*, *Rêveries d'un promeneur solitaire*, the first part of *La Nouvelle Héloïse*)—and these writings are the best known by the general public or shall we say the ones preferred?—Rousseau *the man was romantic.*

(2) Those works in which the philosopher speaks, and gives his message. (*Discourses*, *Lettre à d'Alembert*, most of *La Nouvelle Héloïse*, *Emil*, *Le Contrat Social*.) That message is the one of the *moral nature* which we have described.—Rousseau *the philosopher is anti-romantic.*

Now we will not say that Rousseau the man did not at times creep in the writings of Rousseau the philosopher; but this is accidental. Let us moreover add this consideration that it was exactly because Rousseau the man was so aware of the shortcomings and the dangers of his *romantic natura* that he felt that he ought to preach—for the good of his fellowmen—with particular force the gospel of the *moral natura.*

But let us come to another cause that brought about what is considered by the writer as a misinterpretation of Rousseau —it is a deeper cause and which, although it cannot quite

justify the position of our modern Rousseauphobes, can to a certain point explain it.

To understand it, we must recall what may be defined the philosophical atmosphere at the time of Rousseau's writings. The eighteenth century had given great momentum to the philosophical revolt known at first, early in the seventeenth century as 'libertinism,' and later under the name of Romanticism: but the philosophy of life that prevailed even then as a firmly established tradition and was strictly adhered to by those who had charge of the moral leadership of the people, *i.e.* the representatives of the Church, was that human nature, considered from the point of view of ethics, was bad, and that the one thing to be achieved in life was to control passions; or, to come back once more to the vocabulary used in this study, to subdue romantic nature and obey moral nature.

Now, while this doctrine as far as the actual behaviour of man is concerned, agreed pretty well with the ethics of many non-Christian philosophers, of many ancient Greeks or Latin moralists, for instance, there was, however, a very essential difference, in as much as the Christians recommended not only the control of nature but the negation of it; that is to say that the Christian moralists (chiefly for reasons of a practical character but which cannot be stated here) condemned not only erroneous ways of enjoying life, but, moreover, taught that enjoyment in itself was wrong, and suffering in itself was superior. This distinction is absolutely capital, for it was against this doctrine of condemnation of pleasure itself that the philosophers of the eigtheenth century first rose in protest; and while some summarily declared that the best way to get rid of the false idea was to dispense with God altogether, some (like Rousseau) maintained that a God of love, as the one preached by the Christians, could not possibly take pleasure in the sufferings of His creatures—no matter at what time, in what form or for what purpose. But they were *all* one in the cry: let us stand by man for that nature which calls for pleasure and avoids suffering as much as it is possible *in this world*.

This was the starting point; but now this path once entered, it leads to a bifurcation: after that affirmation that man must follow nature which does not forbid, and even favors, enjoyment of life, the question arises: which is the way to enjoy life? And it was to this second query that many carried away by their determination to crush completely the advocates of ascetism, answered: let us still follow nature, let us follow it to the very end; and even when it suggests to listen to the direct appeals of our passions. . . . And it was this attitude that led ultimately to Romanticism which in turn led so many to grief—as was told in verses or proses never to be forgotten by the most talented of those Romanticists, Chateaubriand and Mme. de Stael, Musset, Vigny and George Sand, etc.

But a few were not carried away, but kept true to the wisdom of Socrates and ancient sages. And they also could claim *nature*, however, as we have explained: *moral nature*. They therefore said: Let us follow *romantic* nature as long as it tells us to enjoy life; but let us cease to follow nature when it tells you that the road to happiness is found in yielding to passion; here, follow *moral* nature which opposes *romantic* nature.

That Rousseau belonged to the second class of thinkers, the class which *did* make the difference between the two phases of the cry 'return to nature'—the one just saying *be happy*, the second *be happy in yielding to passions*,—we have shown above. It must be said that most eighteenth century "Philosophers" recoiled when it came to draw all the conclusions of their premisses and even warned against the idea of simply yielding to nature and to whatever it suggests. Even the most radical did stop at some point before reaching the extreme limits;—as Helvetius, d'Holbach and of course Voltaire who takes back in his *Poème de la loi naturelle* and in *Uranie* much of what he had said in *Le Mondain*. The one who went furthest was Diderot in his famous *Supplément au Voyage de Bougainville* (which bears as sub-title: "*Dialogue sur l'inconvénient d'attacher des idées morales à certaines actions*

qui n'en comportent point"). Most of them made a beautiful mess of it: *e.g.* D'Hollbach *Système de la Nature*, II, 446. "O nature, souveraine de tous les êtres, et vous ses filles adorables, vertu, raison, vérité, soyez à jamais nos seules divinités." Rousseau himself is guilty of grave confusion, *e.g.* in his famous passage "Conscience, conscience divine instinct. . . ." He speaks here—as in other places too—as if moral conscience was innate in man, belonged to the primitive, undeveloped nature. If he believed it, he was not in agreement with the rest of his psychology and philosophy. As a matter of fact he never directly discussed the question of innateness of conscience, but whenever he had to deal with it indirectly, he never failed to associate moral nature with reason—especially in *Emil*—and when he opposed the "Philosophers" who spoke lightly of "moral conscience," he used rational arguments to refute them and vindicate the doctrine of inhibition of romantic nature by moral nature.

There is so much misunderstanding here as regards Rousseau's attitude, that a rapid *mise au point* is in order: What *did* Rousseau say regards to nature; and—which is probably more important still—what did he not say?

It has been repeated over and over again that Rousseau claimed that man is good by nature, not only in the sense that the nature of man is good, but that man is good at the start, and in his state of nature;—this is indeed the theory most intimately attached to Rousseau's name in the eyes of posterity. He discussed this idea in his *Second Discourse*. Now the truth of the matter is, that in the text of this *Second Discourse*, there is not one passage which supports the claim that he himself accepted that idea. All he said was that man is *not*—as the Church had claimed—*bad* by nature; but there is a long way between the statement that man is not bad and the other that he is positively good. It must be admitted that Rousseau, later, alluding to that discussion asserted that he had proved that man was *good* (not only "not bad"); and while of course, one cannot praise Rousseau for this heedlessness, on the other hand the fair thing to do is to go back to the

actual writing in which the problem is faced and discussed. (See for full discussion by the writer: *Revue du 18e siècle*, octobre—novembre, 1913, and his book *La pensée de Rousseau*, Alcan, 1929, pp. 174–179; IIe Partie, ch. II, § 3.)

It has been further maintained that Rousseau had proposed to man to 'return to nature' in order to find happiness. This again he never did. He said that man was less happy in civilization than man in the state of nature; he even had moments of hesitation whether to give the advice of returning to nature, but he never gave it, and he resented when people accused him of having done so. The important point here is that he was right in resenting it, and that he could not consistently advocate such an idea,—since he had never said that man in the state of nature was happy (no more than he had proved he was good); he had only maintained that that man was less unhappy. Therefore if man was not happy in the state of nature, how should Rousseau recommend to go back to that state? He has described the state of nature as one in which man only knows of absence of various sufferings connected with social life, but he very positively says that to know of real human and superior pleasures, man must first leave the state of nature and develop social life.

What then *did* Rousseau say? He did say as we have stated before: (1) that man is not naturally bad morally speaking; and moreover (2) that man is right in listening to the natural desires to enjoy the good things that life may offer—and on these two points he disagreed with St. Paul, Augustine, Luther, Calvin, Pascal, in short with the christian theology that still obtained at the time, while he agreed with the new "philosophers" who ever since the Renaissance had fought the idea of the *a priori* superiority of suffering over enjoyment.

(3) And then Rousseau did not stop there; he continued his investigations and he added this: If man is right in listening to the natural call inside of him for happiness in this world, man is mistaken if he listens to the luring of nature *when nature invites him to yield to passion*—and in this he agrees

with St. Paul, Augustine, Luther, Calvin, Pascal and with Christian ethics, while he no longer agrees with the new "philosophers" of nature at the time; and in this moreover he is absolutely antagonistic in advance to the coming *Romanticists.* Although the latter often did claim Rousseau as their forerunner, they had no right to do so. And this necessity of not yielding to human passions Rousseau emphasized very much indeed, not only because man is weak but because of the danger of being himself misunderstood: having said listen to nature and rejoice, people might infer that he meant (as said the "philosophers") listen to passionate nature.[1]

Now of course, while the representations of the Church recommended moral discipline, merely in view of a future life, Rousseau recommended it (as the Ancient philosophers did) because it seemed to him a condition for present happiness—and one easily understands on that score, the hostility of the Church towards Rousseau: should Rousseau have succeeded he would have taken away from the priests the hold on the people by means of the belief in an after-life. But Rousseau in his *Profession de Foi du Vicaire savoyard* would have had a ready answer; he was well aware that the observation of the rules of *moral nature* are not enough to bring about happiness in all cases; and that to achieve justice there is still enough to be done for an after-life.

[1] Here the objection must be waived again to which we alluded above, namely that one will find plenty of quotations from these "philosophers" which indicate they often fully realized that it was unwise to surrender entirely to natural instincts; but the point is that when they made such statements it was in contradiction with their fundamental doctrine and that—rightly—Rousseau judged their doctrine as if consistently applied.

DIFFERENT RATES OF GROWTH AMONG ANIMALS WITH SPECIAL REFERENCE TO THE ODONATA

By PHILIP P. CALVERT

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Rearing of Two Species of Odonata

Two methods of ascertaining the duration and the differences in size and in structure of the successive instars of insects have been discussed by Wesenberg-Lund (1913, pp. 374 *et seq.*) with special reference to the Odonata, that of

rearing known individuals in aquaria and that of comparing specimens taken in nature at regular intervals (analysis of populations). He has set forth the obstacles which he met in following each of these. Unquestionably the former method —that of rearing—gives the most dependable results when its technical difficulties are overcome.

In the summer of 1925 the writer began the attempt to raise *Nannothemis bella* (Libellulinæ) from the egg; primarily for the purpose of obtaining the morphological differences between the successive larval instars. As the experiment went on, the idea of recording some data on the rate of growth was conceived and carried into effect. In the following summer a similar trial was made with *Anax junius* (Æshninæ). Success was attained in one individual of the latter species and in three of the former. The present paper deals with the growth of these two species and with comparisons with other animals, reserving the morphological descriptions for separate memoirs to be published elsewhere.

1. *Nannothemis bella* (Uhler)

Material and Conditions of Rearing.—Eggs of *Nannothemis bella* were obtained at Chatsworth, Burlington County, New Jersey, on July 18, 1925, the opportunity for visiting this locality being due to Mr. Harold C. Hallock, then of the Japanese Beetle Laboratory of the United States Bureau of Entomology at Riverton, New Jersey. One of the smaller tributaries of the Wading River is dammed near Chatsworth. Among the tufted and partly overflowed vegetation on the right bank above the dam, adults of *Nannothemis bella* were present. I secured a pair which was flying together, the male holding the female by her head while she dipped her abdomen repeatedly into the water to discharge her eggs. Dipping the hind end of the abdomen of this female in a vial of water, I obtained upward of 50 eggs which I took to my home. Here they began to hatch about August 5, 1925, and continued to do so at least until August 27. The pronymphal stage (Balfour-Browne, 1909) was not observed. Soon after the

larvæ appeared they were removed to various small dishes. At least fifty larvæ hatched from the eggs; on August 17–18, a number of them made their first molt. From August 18 to 28, nineteen of the larvæ were isolated each one placed in a glass salt cellar, 32 mm. diameter, 13 mm. deep (inside measurements) having a capacity of about 5 cc., covered with a sheet of glass, and numbered from 51 to 69 inclusive. In each dish a small quantity of an aquatic plant (*Elodea* in earlier months, *Lemna* in later) was kept growing. Through the nearly three years during which the rearing of *bella* was carried on, the dishes containing the larvæ were kept on the inner sill of a window facing north in my study. From November, 1925, until after June 7, 1928, an additional outer (storm) sash was kept uninterruptedly on this window. The temperature of the room, as indicated by a self-recording maximum and minimum thermometer, ranged from 89° to 50° F. (31.6° to 10° C.).

Most of larvæ numbers 51 to 69 died after they had been isolated ten days, while still in the second instar. Only four of them, numbers 56, 58, 65 and 66, lived on to furnish the data for this paper. Up to September 27, 1925, the larvæ were examined every one to three days; after the latter date, the intervals became longer until from February 7, 1926, they were one week in length. Measurements of the living larvæ were made on August 6, October 27, and November 8, 1925, January 10, February 14, April 18, and April 25, 1926; beginning May 16, 1926, they were continued at weekly intervals to the end of the life of each individual.

Each larva, when measurements were to be made, was transferred from its container by means of a small tin spoon to a watch glass with water and allowed to rest for at least five minutes before measuring. Measurements were made under a Zeiss-Greenough binocular, oculars 4, objectives 55A, with an eyepiece micrometer.

On December 12, 1925, each of the four living larvæ, then somewhat over 2 mm. long, was transferred from the salt cellar to a glass "caster cup," 43 mm. diameter, 13 mm. deep

(inside measurements), capacity 15 cc., with the water and other contents in which it had been living previously. On June 20, 1926, each *bella* was transferred from the "caster cup" to a "nappie" or finger bowl, 78 mm. diameter at bottom, 95 mm. diameter at top and 33 mm. deep. Each "caster cup" and "nappie" was kept constantly covered with a piece of glass.

Food Supply.—The following excerpts from the records kept of each isolated larva give an idea of the food on which the four larvæ of *bella* accomplished their growth. When the larvæ were about one week old, drops of a culture of *Paramecium* were placed in the vessels in which they were living. Soon after, other Infusoria, small copepods, ostracods, rotifers, larvæ of *Anopheles* and other organisms smaller than *bella* were added. For nearly a year from September 19, 1925, the food supply was chiefly the small crustacea some of which bred in the dishes containing *bella*. In August, 1926, Chironomid and Ephemerid larvæ, in September and October 1926, Corixid and Culicid larvæ were given in addition. In late February, 1927, mayfly larvæ were more frequently furnished; although these were taken from a swiftly-flowing stream, they lived in the absolutely still water of the dish for at least eight days and possibly longer. Similar observations were made in connection with other mayfly larvæ, those of the genus *Heptagenia* being much more able to survive in still water than those of *Bætis*, for example.

Special attention was paid, each day that the *bella* larvæ were examined, to noting whether any possible living food material had survived from the previous week and in only three cases was an entire lack of such found (October 17, 1926, *bella* number 66; October 10, 17, 1926, number 56; in two of these cases *bella* was, however, a little larger than at the previous week). The slow rate of development of the *bella* larvæ cannot, therefore, be ascribed to absolute starvation, although it is of course possible that the optimum food was not in the dishes. The hairs on the body of *bella* larva are fairly dense, and to them vegetable debris usually adhered

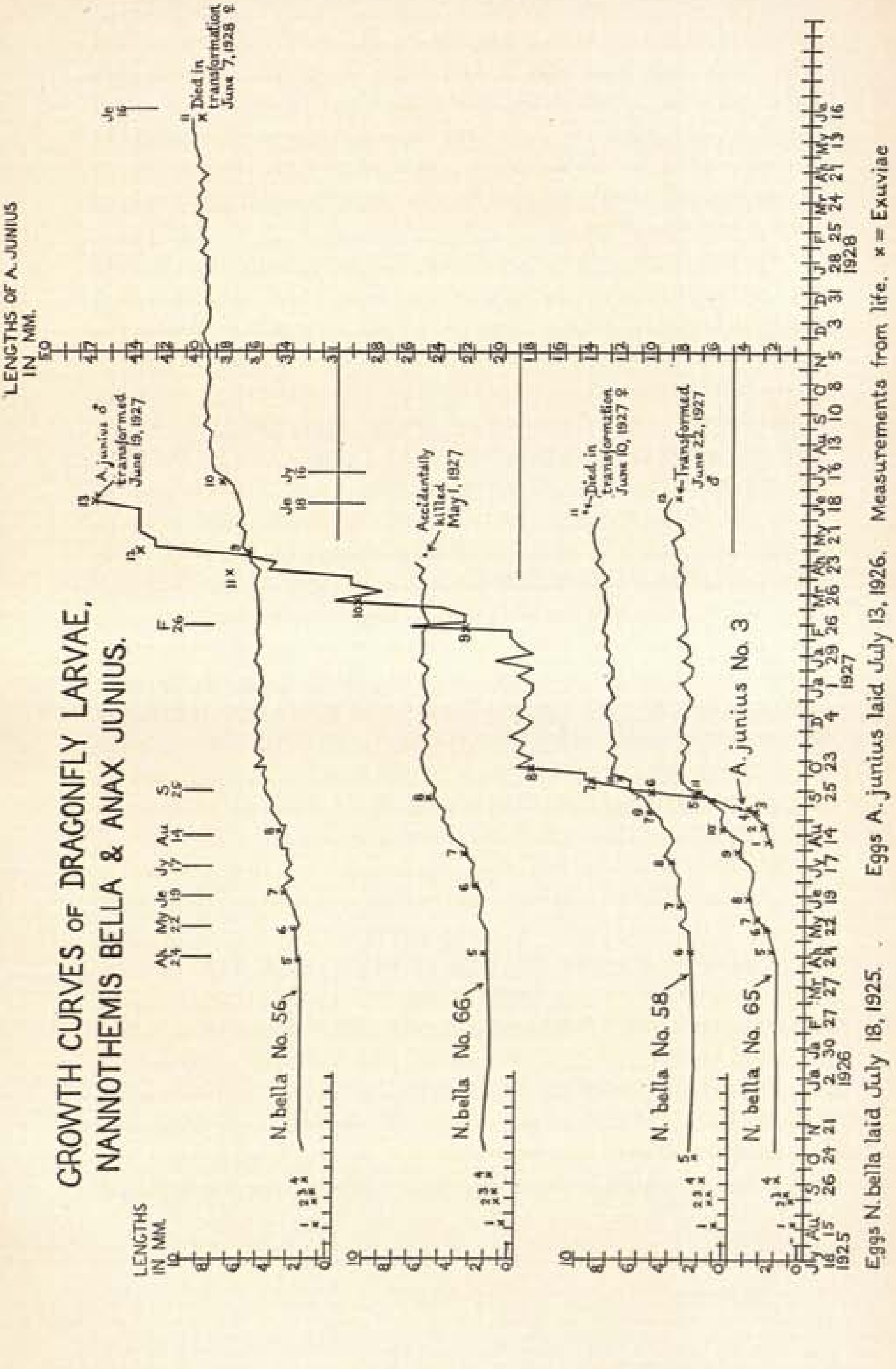

GROWTH CURVES OF DRAGONFLY LARVAE,
NANNOTHEMIS BELLA & ANAX JUNIUS.
LENGTHS IN MM.
LENGTHS OF A. JUNIUS IN MM.
N. bella No. 56
N. bella No. 66
N. bella No. 58
N. bella No. 65
A. junius No. 3
A. junius ♂ transformed June 19, 1927
Died in transformation June 7, 1928 ♀
Accidentally killed May 1, 1927
Died in transformation June 10, 1927 ♀
Transformed June 22, 1927 ♂
1925
1926
1927
1928
Eggs N. bella laid July 18, 1925. Eggs A. junius laid July 13, 1926. Measurements from life. x = Exuviae

Fig. 1.

to such an extent as to hide the body surface. This mass which the slowly moving *bella* carried constantly, served as a part of the food-supply of the small crustacea or of the still smaller animals (infusoria, rotifers) on which the crustacea fed.

Moultings.—The growth curves and moultings (ecdyses) of the four larvæ which survived the second instar are shown in Figure 1. From them it will be seen that two of the larvæ (numbers 65 and 58) completed the larval instars in approximately 23 months from oviposition, one (65) by 12, the other (58) by 11 ecdyses; a third (number 56) occupied 34⅗ months from oviposition to reach the imago stage by 11 ecdyses; the fourth (66), accidentally killed in the ninth larval instar, at the age of 21½ months from oviposition, appears to have been growing at a rate intermediate between numbers 65 and 58 on the one hand and number 56 on the other. As the three larvæ, numbers 65, 58, and 56 reached lengths of 9.49, 9.25 and 9.65 mm. respectively, before transformation, no correlation in size, length of larval life or number of ecdyses is suggested.

From January to April, 1926, the *bella* larvæ were very sluggish, this feature being more marked in numbers 56 and 58 than in the other two. Responses of number 56 to touch stimuli, on March 14 were very slow and slight, but even in this individual the degree of sluggishness varied from week to week.

Transformation.—On June 12, 1927, the wing cases of *bella* number 65 were whitish. On June 22, at 6:30 A.M. (Eastern Standard Time only is used throughout this paper), this larva had crawled on to a stick of wood of 11 mm. diameter and 150 mm. long, whose lower end rested in the water in the dish, in which the larva had lived since September 5, 1926, on which date it had been transferred to a second "nappie." This "nappie" had been placed, a few days previously, in a glass cylindrical battery jar 127 mm. in diameter and 190 mm. high. The stick extended 16 mm. below the water's surface, its upper end reached to 44 mm.

below the top of the battery jar and rested against a strip of copper wire netting (14 meshes to 25 mm.) 100 x 457 mm. inclined at about 70° with the horizon, whose lower end reached to 16 mm. above the water's surface and whose upper end was attached to a framed window screen in front of a closed window facing south. At the time mentioned (6:30 A.M.) the head and thorax of *bella* were above water, the rest of the body submerged, the eyes whitish. Shortly before 7 A.M. *bella* climbed up from the stick to the upper surface of the copper netting to a height of 300 mm. above the water's surface and 146 mm. above the surrounding battery jar. Here it transformed. By 7:15 its body was out of the exuvia [1] except at its hind end, projecting almost at right angles to the exuvia and the netting. At 7:30 it had entirely withdrawn its body from the exuvia and was clinging by its legs to the exuvia and netting. At 8:05 its wings were fully expanded. The general color of the imago during transformation and up to this time was pale green; at 8:40 the abdomen was pale green, the thorax brown, the eyes reddish brown above, the frons pale green. At 9:05 it had spread out its wings into the frontal plane. At 1:40 P.M. the abdomen was blackish. It was kept alive until 5:30 A.M., June 23, and then killed in a cyanide bottle. The morning of June 22 during which transformation occurred was cloudy, with no sunshine.

Larva number 58 was found dead on June 10, 1927, floating in the water of its "nappie." The cuticle of the head had begun to open and part of the imaginal eyes were uncovered, so that death had evidently come in attempted transformation.

Larva number 56 was clinging to a stick in its dish on June 3, 1928, but was removed therefrom for measurement (9.65 mm. long) in a watch glass as usual. On June 5, it had crawled up on the stick entirely out of the water and so remained during June 6. On June 7, at 8 A.M. it was on the stick about 50 mm. above the water's surface. At my next

[1] Because of the convenience of having a singular form, I have adopted, here and elsewhere, as a technical zoölogical term *exuvia*, although aware that such does not exist in classical or post-classical Latin.

opportunity to observe it, 4:30 P.M., it had crawled from the stick on to the copper wire netting, arranged as described above for larva number 65, and partially emerged, the head, thorax, most of the abdomen and the bases of the wings being out of the exuvia, while the remainder of the wings and the apex of the abdomen were still within the exuvia. Those parts out of the exuvia were fairly well colored, the bases of the wings were hardened, the abdomen not fully extended. The exuvia was not clinging to the netting, so that the attachment made by the larva to the netting was, therefore, probably not sufficient to hold the larval legs on the netting when the imago endeavored to pull its wings and abdomen free of the exuvia. During the evening of June 6, the jar (with its copper netting) in which this larva had been living since June 26, 1927, had been transferred to a southern window in the same room, so as to expose *bella* to some sunlight during daylight hours. Number 56 was alive at 6 A.M., June 8, but dead at 6:30 P.M. The exuvia, before any preparation of it for preservation was made was 8.83 mm. long (mean of four measurements). The imago was then pinned with its wings and abdomen still partly within the exuvia.

2. *Anax junius* (Drury)

Material and Conditions of Rearing.—Eggs of this species were obtained at Primos, Delaware County, Pennsylvania, July 13, 1926, at 3 P.M. A pair of the adults was observed resting on some water-logged wood, in a shallow pool in an abandoned brick yard. The soil is largely clay. The female was apparently ovipositing in the wood. The wood was secured, although the insects escaped. The exact part in which eggs were, seemingly, being inserted had been noted; this was broken off in several pieces, and taken in a jar with water to my home. It was kept continuously in water until August 21, 1927. On August 1, 1926, four larvæ, numbers 1 to 4, were found in the water around the wood and removed; one (number 5) was found on August 5, and removed.[1] None

[1] Among my notes of earlier years I find the following: Eggs of *Anax junius* obtained from a female at Tinicum Island, Pennsylvania, June 28, 1889; began hatching July 6.

were found in examinations on August 8, 12 and 18. On the last date, after the examination, the water was poured off and the wood placed in fresh water. On September 11, one dead (number 6, younger) and one living (number 7, older) larva and two exuviæ were found in the water. Other examinations were made on September 12, and October 24, 1926, May 8, June 19 and August 21, 1927, but no further larvæ or exuviæ were found. In the examinations of 1927, at least, each piece of wood was examined separately and carefully in clear water, as well as the water in which the wood had been lying. The possibility that eggs of *A. junius* or of other species had been laid in this wood previous to July 13, 1926, is, of course, not excluded.

Larvæ numbers 1 to 4, first observed on August 1, 1926, may have emerged from the eggs at any time between July 26 and August 1. Each of them was placed in a separate glass container covered with a watch glass on that day. Observations on the seven larvæ mentioned are summarized in the following table:

TABLE 1

EARLY INSTARS OF ANAX JUNIUS FROM EGGS PRESUMABLY LAID JULY 13, 1926

Larva No.	Found	Length of Living Larva in Mm.	Instar when Found	Next Exuvia Found	Length of Living Larva in Mm.	Subsequent Fate
1	Aug. 1, 1926	1.88	1st	Aug. 8, 1926	2.13	Dead Aug. 15, 1926
2	do.	2.37	1st	Aug. 5, 1926	2.29	do.
3	do.	2.12	1st	Aug. 8, 1926	2.05	Reared to imago.
4	do.	2.45	2d	Aug. 5, 1926	2.95	Dead Aug. 13, 1926
5	Aug. 5, 1926	2.45	2d	Aug. 14, 1926	2.70	Dead Aug. 15, 1926
6	Sept. 11, 1926	(dead)				
7	do.					Dead Sept. 12, 1926

It is possible that numbers 6 and 7 are not *Anax*.

From an examination of this table it will be seen that the possible minimum duration of the first instar was four days (number 2), of the second instar four days (number 4), but it may have been longer in each case. Measurements were made by the same means and method as for *N. bella*, *anteà*.

The remainder of this account of the development and growth of *Anax junius* is based solely on larva number 3. On September 17, 1926, it was transferred from a "caster cup" to a "nappie" containing some aquatic plants: *Elodea*, *Lemna*. During the winter of 1927 the larva was frequently transferred to a stender dish for an hour or two for the purpose of making measurements, counting the rectal respirations, or for other observations, and then returned to the "nappie." On March 20, 1927, *junius*, the water in which it had been living, Lemna and other plants were transferred from the "nappie" to a "battery jar" 145 mm. in diameter and 160 mm. high, and covered with a sheet of glass as before. On May 8, a stick of wood on which *junius* might climb was placed in the jar.

Food Supply.—All the larvæ of *junius* when isolated were supplied with very small Crustacea and other animals were often included. On August 14, 1926, number 3 (2.45 mm. long) was observed to catch, after three attempts, a rotifer about .08 mm. long and to eat it. The next day it was seen to swallow an ostracod about .1 mm. long, which was passed back to the crop in the metathorax, apparently without being crushed or subdivided. A second of the same size was similarly treated, the two lying side by side in the crop. After half an hour, they were still visible there. *Junius* then snapped and caught a third of about the same size which likewise was passed to the same location. Some action of the mandibles was performed on all three, but not apparently enough to alter the shape or dismember the ostracods. Again on August 18, number 3 was seen to eat three small ostracods. On August 25 it seized and ate a female copepod (bearing eggs) almost as long as the width of its own head; it soon caught and ate a second copepod. On August 27 it ate a copepod longer than the width of its own head. On August 29 it chewed an ostracod, .33 mm. long, with its mandibles, while holding it by the mask, but could not penetrate the shell of the victim and dropped it; the latter lay on the bottom some little distance below *junius* and was able to move

sometime later. An ostracod .57 mm. long, was seized on September 11, 1926, but was dropped again—too smooth to be pierced? Two similar observations were made on September 13; an Anopheles larva and a Dixid larva were added this day and on September 14, *junius*, attracted by a movement of the Dixid larva across the dish and near to itself, was seen to slink up very slowly and seize the Dixid near the middle of its length, shift it so that it could begin feeding on the Dixid's head, holding it with the mask and chewing it with the mandibles. The Dixid was about 4.09 mm. long, *junius* before eating it 5.73 mm. long, after eating it 6.22 mm. From this time on *Culex* and *Anopheles* larvæ, small Corixids (including larvæ), *Asellus*, Daphnids and mayfly larvæ were added. Copepods and ostracods continued to be present and probably bred in the jar. *Junius* was seen to seize and eat a corixid on September 22, in spite of much struggling of the latter; the anterior three fourths of the abdomen, chiefly on the ventral side, were consumed, the head and thorax abandoned. The hemelytra of the corixids were frequently found floating on the water's surface—the remainder of these insects having been devoured. *Junius* seemed to pay no attention to the *Aselli*, even when they crawled beneath its body and they were removed from the jar after a month. From March 13, 1927 on, mayfly larvæ, especially of the genus *Heptagenia*, were almost the only food given.

No correlation can be traced between the bulk of food supplied to *junius* and the growth in length of the latter, as is evident from an examination of Table 2. This table does not include the small copepods and ostracods which were living and breeding in the jar, but it is usually supposed that *junius* larvæ, in their later stages at least, prefer larger food than these small crustacea.

In explanation of the column headed "Living Food Remaining from Previous Feedings," it should be said that *fragments* of food left from previous feedings were frequently found in the jar and removed but less often (October 14, 3 Corixids; October 17, 1 Corixid; October 24, 3 Corixids;

TABLE 2

GROWTH OF *Anax junius* LARVA (NUMBER 3) IN RELATION TO ITS LIVING FOOD AND THE FATE OF THE LATTER

Date	Length of *junius* in Mm.	Exuviæ Found	Living Food Remaining from Previous Feedings	Living Food Added
Sept. 17, 1926	5.97			8 *Culex* Larvæ (ca. 2 mm. long) 1 Culicid pupa, 1 small Corixid
" 19, "	7.85		0	3 Corixids (2 of them larvæ)
" 20, "	8.3	(5th exuvia)	0	16 *Culex* larvæ, 1 *Anopheles* larva, 1 Gammarid
" 22, "				4 small Corixids
" 24, "	10.96	(6th exuvia)		Do.
" 26, "	11.04			5 small Corixids, 2 *Anopheles* larvæ, 1 Mayfly larva
" 28, "			2 Corixids, 1 Anopheles larva	
" 29, "				9 small Corixids
Oct. 3, "		(7th exuvia)		
" 4, "	14.5			16 small Corixids, 3 *Asellus*
" 7, "				6 small Corixids
" 10, "			2 Corixids	7 *Culex* larvæ
" 12, "	14.5		2 Asellus, 1 *Culex* larva	10 small Corixids, 1 Zygopterous Odonate larva
" 14, "				5 Odonate larvæ
" 17, "	19.0	(8th exuvia)	1 Corixid, 1 Zygopterous larva	6 Odonate larvæ
" 20, "				13 Odonate larvæ, 1 *Asellus*
" 22, "				9 Odonate larvæ, Daphnids
" 24, "	18.		3 Corixids	6 Odonate larvæ, 2 *Asellus*
" 29, "			Several Corixids	2 *Asellus*
" 31, "	19.		2 Corixids, 2 *Asellus*, 1 Zygopterous larva	15 large Daphnids

TABLE 2.—*Continued*

Date	Length of *junius* in Mm.	Exuviæ Found	Living Food Remaining from Previous Feedings	Living Food Added
Nov. 7, 1926	18.5		3 *Asellus*	Daphnids
" 9, "				1 Mayfly larva, 1 Caddis larva
" 10, "				1 Mayfly larva, 1 Tipulid larva
" 14, "	19.5		3 *Asellus*, 1 Mayfly larva, 1 Tipulid larva	Daphnids
" 21, "	18.5		3 *Asellus*, 1 Tipulid larva	1 Mayfly larva
" 28, "	18.0		3 *Asellus* (removed)	5 Mayfly larvæ (3 to 10 mm. long) 1 Dixid larva, Daphnids
Dec. 5, "	19.		1 Dixid larva	
" 7, "				6 Mayfly larvæ
" 12, "	18.5		3 Mayfly larva	
" 19, "	18.5		Do.	
" 26, "	18.0		1 Mayfly larva	
Jan. 2, 1927	18.0			3 Perlid larvæ
" 9, "	19.0		1 Mayfly larva, 2 Perlid larvæ	1 red Chironomid larva
" 16, "	18.0		1 Mayfly larva, 2 Perlid larvæ, 1 Chironomid larva	
" 23, "	20.5		1 Mayfly larva	
" 30, "	18.		1 Mayfly larva	6 Mayfly larvæ, 1 Caddis larva, 2 Chironomid larvæ
Feb. 6, "	19.		1 Mayfly larva, 1 Chironomid larva	
" 13, "	19.5			2 small Zygopterous larvæ, 1 Daphnid
" 15, "				4 Mayfly larvæ, 1 Stonefly larvæ
" 20, "	19.5		1 Mayfly larva	

TABLE 2.—*Continued*

Date	Length of *junius* in Mm.	Exuviæ Found	Living Food Remaining from Previous Feedings	Living Food Added
Feb. 25, 1926........	26.	(9th exuvia)	1 Mayfly larva	3 Caddis worms
" 27, "	22.5		1 Mayfly larva	
March 6, "	22.5		None	5 Mayfly larvæ, 1 Caddis worm
" 13, "	24.		None	7 Mayfly larvæ
" 20, "	31.	(10th exuvia)		11 Mayfly larvæ, 1 Zygopterous larva
" 27, "	28.		None	6 Mayfly larvæ
April 3, "	30.		None	11 Mayfly larvæ, 1 Caddis worm
" 10, "	30.		None	7 Mayfly larvæ
" 17, "	35.5	(11th exuvia)	None	15 Mayfly larvæ 10 to 15 mm. long
" 24, "	35.		None	10 Mayfly larvæ
May 1, "	37.			8 Mayfly larvæ
" 8, "	43.	(12th exuvia)		10 Mayfly larvæ, mostly less than 10 mm.
" 15, "	43.		None	13 Mayfly larvæ
" 22, "	44.		None	18 Mayfly larvæ, from 7 mm. up
" 29, "	44.		None	24 Mayfly larvæ, mostly more than 12 mm.
June 5, "	44.		None	16 Mayfly larvæ
" 12, "	44.		None	7 Mayfly larvæ
" 19, "	47.	(13th exuvia) (Transformation)		
" 21, "			1 Mayfly larva	

October 31, 2 Corixids; November 21, 2 Corixids; December 19, 1 small Mayfly larva; February 27, 2 Caddis worms; April 10, 1 large Mayfly larva) *entire*, uninjured, *dead* insects.

Transformation.—For some days previous to June 12, 1927, *junius* was observed to be resting on the stick but keeping the apex of the anal pyramid above the water's surface. On June 12, it was measured as it clung to the stick from which it was not removed for fear of disturbing transformation, apparently imminent; it was 44 mm. long. During the following week it clung to the stick, changing its position from time to time, usually with its head down and only the anal pyramid above the water's surface. On June 19, at 12:15 P.M. still on the stick it had the head up and above the water's surface, the rest of the body submerged. It transformed to the imaginal stage between 8 and 10:45 P.M. From about 8 P.M. to 9:30 P.M. the electric light in the room was shining but whether *junius* came out of the water during this period was not observed. At 10:45 the body and wings were fully expanded.

The jar in which *junius* had been living since March 20, 1927 (see page 236), had in it, at the time of transformation, water to a depth of 28.5 mm. at the circumference of the jar (a little shallower at the centre by reason of the convexity of the bottom). In the jar was a stick 385 mm. long, 12 mm. in diameter, placed slantwise on the bottom and leaning against a strip of copper wire netting, of the same kind as that used for *N. bella* (see page 233), 100 by 457 mm. This netting reached to within 19 mm. of the water's surface and extended 343 mm. above the top of the jar and was hooked on to the top of a wooden-framed window screen on the inside of a window whose sashes were kept closed. The jar stood for more than a week previously on the inner sill of this window, having a southern exposure and with outside slatted shutters which cut off some light. *Junius* climbed up the stick and on to the under side of the copper netting, which formed an angle of about 60° with the horizon, to a height of 300 mm. above the water's surface and 160 mm. above the top of the

jar, as it was there that its final exuvia was found. Between 11 and 11:30 P.M. it was transferred to a large paper bag in which it remained alive until June 21, at 9 A.M., when, as it began to flutter in the bag, it was removed to a glass jar with netting on top and placed in front of a southern window. At 3:20 P.M. the pale colors on abdominal segments I and II were pale blue and green, on III to IX reddish, on X pale bluish green, the mid-dorsal stripe on III to X brown. This imago was subsequently killed and preserved.

Comparisons of Growth in Length Between Nannothemis Bella and Anax Junius (Odonata)

The more remarkable facts disclosed by an examination of the accompanying graph of the growth curves of the larvæ of *Nannothemis bella* and *Anax junius* (Fig. 1) are:

1. The larger species required but 341 days from the date of presumed egg-laying to reach its full larval length of 44 mm.,[3] while the smaller, to attain a maximal larval length of 9.25 to 9.65 mm., occupied from 692 to 1055 days. Yet the individuals of the two species were reared side by side at the same window in the same room, exposed to the same temperatures and to the same illumination. Correlated with the slower growth of *N. bella* are its more sluggish movements under all conditions. This latter characteristic was remarked by Weith (1901), the first to find the larvæ of this species. He says: "The nymph of *N. bella* is the most sluggish insect I know of: Stratiomyia and Odontomyia larvæ, which are abundant in the same places, are race-horses in comparison. Removed from the water, the nymph clings closely to grass or debris of exactly its own color and does not stir even after

[3] That species of this genus may develop in a still shorter period is shown by the experiments of Portmann (1921), on the European *Anax imperator*. He put a larva, which was in the 11th larval instar and had lived during the summer in water of temperature 18°–20° C., into a temperature of ca. 25° C. on October 20th. This moulted on November 19 (completing its 11th instar) and transformed on January 2 (completing its 12th instar), 206 days from oviposition. Another larva in its penultimate instar, which had been living in water of 8°–10° C., was put into 25° on January 9, moulted February 1 and transformed on March 9. A third larva in the same instar, and which had likewise been living in water of 8°–10° C., was put into 25° on January 9, moulted February 5 and transformed March 14.

letting this dry; so it is hard to see, and everything has to be picked over very carefully." On the other hand the larva of *Anax junius* is alert and active. Needham (1901) wrote of it: "It clings to water weeds nearer the surface, usually, than the bottom, in an attitude of alertness with head poised low and abdomen slightly elevated. Locomotion is relatively rapid, either by walking or by swimming by ejections of water from the respiratory chamber."

The area of distribution of *N. bella* is from Maine to Georgia to Indiana and Ontario, in the Alleghanian and Carolinian zones, while that of *A. junius* is much greater, being entire North America, the Hawaiian Islands and perhaps the western coast of Asia (Muttkowski 1910). Kennedy (1922) and Miss Lamb (1925) have suggested that the widespread distribution of the species of *Pantala* is due to their ability to mature quickly in shallow pools; the same argument may be advanced to account for the greater area occupied by *junius* contrasted with that of *bella*.

2. The four larvæ of *N. bella*, under identical environment and with apparently identical food, grew at different rates of speed. On October 24, 1926, when they were 463 days old, reckoned from the date of oviposition, they stood as follows:

No. 56, 4.75 mm. long, 49.2 per cent. of final larval length, had moulted 8 times.
No. 66, 6.38 mm. long, ? of final larval length, had moulted 8 times.
No. 58, 7.53 mm. long, 81.4 per cent. of final larval length, had moulted 10 times.
No. 65, 8.00 mm. long, 84.3 per cent. of final larval length, had moulted 11 times.

So far behind the other three did number 56 lag that it required one more year to reach the imago stage than did numbers 58 and 65. The total number of moults made by number 56 was, nevertheless the same (11) as those of number 58, while number 65 which transformed only 10 days later than number 58 continued its lead of one moult (12 in all).[4]

[4] See the analogous results of the Severins (1911) with *Diapheromera femorata*, of Forbes (1920, p. 194) with pupæ of the codling moth, of Cotton (1927) with *Tenebrio*.

All three larvæ of *bella* stand behind the larva of *junius* with its thirteen moults.

3. The temperatures in the room in which these larvæ of *N. bella* and *A. junius* were reared were doubtless higher in the winters (never lower than 10° C. = 50° F.) than those to which these larvæ are exposed in nature at this season. Nevertheless all five larvæ show a halt in growth in the winters, from which *junius* made recovery earlier (latter half of February) than *bella* (April or May). Comparing the single winter of *junius* with the last (second or third) winter of those *bella* which reached transformation, *junius* made five subsequent ecdyses, *bella* none.

4. The "step-wise" nature of the growth curve of *A. junius* is very marked in the latter six instars, less so in those which precede; it has already been figured for the last two moults of this species, both as respects weight and size, by Shafer (1923). The steps are not as evident in the curves of *N. bella;* this is due partly to the smaller size of the insect, rendering them not as distinct on the scale on which the curves are drawn, partly to the great fluctuations in length which the same individual shows from time to time, which in turn may be caused by irregular eating correlated with the sluggishness of this species.

Rates of Growth of Animals in General—Continuous and Discontinuous Growth

In most groups of animals growth in weight and in size (volume) is continuous. As Olmstead and Baumberger (1923, p. 279), Eidmann (1924*a*) and Teissier (1928*a*) have emphasized, this statement holds true for growth in weight, but not for growth in length, of Arthropods. In this class, linear growth is discontinuous, occurs chiefly and more or less abruptly at intervals corresponding to sheddings of the external chitinous cuticle so that the length-growth curve has been compared by Shafer (1923, p. 307) and by Eidmann (1924*b*, p. 581) to a series of steps and the profile of stairs. Eidmann's (1924*a*, p. 125, 1924*b*, pp. 581, 582) statement for

the walking stick, *Dixippus morosus*, is: Growth in length occurs solely at the moults; the larva does not grow during the time between moults, and in this Ziegelmayer (1925, p. 544) and Teissier (1928) agree.

Gros (1925, p. 82), describing the larval development of the mayfly, *Ecdyonurus forcipula*, likens the first six stages to steps, those following to an inclined plane. The former he delimits by the moults which separate them, in the latter he groups "in the same stage the intervals between three or four moults, since the intermediate moults do not lead to important modifications." The ontogeny here would appear to pass from discontinuous to continuous growth.

Evidence exists however, that growth does take place between the moults of Heteropterous, Coleopterous, Lepidopterous and Dipterous larvæ.[1] The length-growth curves for the larvæ of the blow fly, *Calliphora erythrocephala*, published by Musconi (1925) are of such shape that from them one can form no true idea of the number of moults through which this insect passes. In fact, moults are nowhere mentioned. According to the data of Hewitt (1914) there are three larval stages, so that Musconi's results clearly show growth between moults.

Fischel (1929) states that in some orb-weaving spiders the cephalothorax grows in length during and immediately after moulting, while the abdomen becomes larger in the intervals between moults shrinking again at the ecdyses; the total impression produced on the observer and as shown by his length-growth curves (p. 447) is that of continuous growth.

For the larvæ of *Nannothemis bella* our measurements show distinct increases in length between moults while for that of *Anax junius* the changes, other than at ecdyses, are negligible (cf. Table 2). The growth curves of these two

[1] *E.g.:* In the Heteroptera, the larvæ of *Jalysus spinosus*, *Sinea diadema*, *Coryzus hyalinus* and *C. sidæ* (Readio, 1923, 1924, 1928, together with an explicit statement in a letter of May 20, 1929, to the writer). For Coleoptera, the larvæ of *Dytiscus marginalis* (Blunck, 1923, pp. 322, 323, figs. 23, 24; cf. Eidmann, 1924*b*, p. 582), *Carabus granulatus* (Oertel, 1924, pp. 327, 344–345) and *C. cancellatus* (Kirchner, 1927, pp. 505–507). As a Lepidopter, the cotton bollworm, *Heliothis armigera* (Quaintance & Brues, 1905, pp. 57–62, 64–66). Among Diptera; *Calliphora erythrocephala* (Musconi, 1925) and *Drosophila melanogaster* (Alpatov, 1929, Figs. 13, 15).

species (Fig. 1) show many minor fluctuations between moults. These fluctuations are in part, at least, due to the ingestion of definite amounts of food and to subsequent egestion. Thus, on September 14, 1926, the larva of *Anax junius* was measured, found to be 5.73 mm. in length and was observed to eat a Dixid larva, 4.09 mm. long, after which its length was 6.22 mm.; on September 17th its length was 5.97 mm. The sluggishness of the larvæ of *Nannothemis bella* affects their feeding, with consequent fluctuations in their length. Blunck (1923, p. 322) has given a similar explanation for fluctuations in the length-growth curve of the larva of *Dytiscus marginalis*, Eidmann (1924*b*, p. 582) for *Dixippus morosus*, and Shafer (1923, pp. 308–309) for those in the body-weight growth curve of *Anax junius*.

Moults not Absolute Indicators of Biologic Age

The idea is widespread that moulting in Arthropods is a direct consequence of growth and is due to increasing pressure of the body within the cuticle bursting the armor which limits enlargement. From such a viewpoint, each successive moult opens the way to further expansion and is thereby a milestone marking approach to the final size of the organism. To say of a given individual that it has passed its third or its sixth ecdysis should give a definite indication of its biologic age. Considerable evidence exists against these views.

Effects of Starvation and of Humidity.—The observations and experiments of Riley (1883) on the larvæ of the beetle *Trogoderma tarsale*, of Wodsedalek (1912, 1917) on the same species, of Chapman (1920) on *Tribolium confusum* (Coleoptera), of Singh Pruthi (1925*b*) and Teissier (1928*b*) on *Tenebrio molitor*,[1] of Titschack (1926) on the caterpillars of the clothes moth, *Tineola biselliella*, agree in this: That starvation, more or less complete, and under appropriate conditions, results in decreased size, prolonged through months or even years, accompanied by moulting, with a progressive *diminution* in size of the shed skins. Subnormal

[1] Von Lengerken (1925) also has produced supernumerary moults in *Tenebrio molitor* although the cause is not clear.

food supply or high humidity, resulted in an additional moult in the caterpillars of *Heliothis armigera* (Quaintance & Brues, 1905, p. 67) and *Pieris brassicæ* (Singh-Pruthi, 1925*a* and *b*) and in the silk worm (Nagamori, 1927). Instances of Lepidoptera brought together by Ripley (1924, p. 280) do not all point the same way: 100 per cent. humidity reduced the number of moults by one in *Eacles imperialis*, *Antheraea mylitta* and *Agrotis ypsilon*, but increased the number by one in *Polia renigera*.

Effects of Temperature.—The Severins (1913) interpreted their experiments on the walking stick, *Diapheromera femorata*, that low temperature has a tendency to decrease the number of moults, while a high temperature increases the number. The very interesting work of Ludwig (1928) shows that the resting stage of the Japanese Beetle (*Popillia japonica*) which, under field conditions in New Jersey, occurs in the third larval instar, may be shifted into the second or the first by exposure to lower temperatures.

Moulting without Growth.—Dürken (1923, pp. 479–480, 547) in describing the larval stages of the may fly, *Ephemerella ignita*, points out that there was an actual decrease in body length in the third and in the ninth instars and explains this as due in part to active differentiation of muscles during which growth is delayed. Some caterpillars of *Tineola bisselliella* moulted as many as eight times, at intervals of two to three days, without any demonstrable differences in the size of the moulted head capsules (Titschack, 1926, p. 551). Larvæ of European species of *Zygæna*, which pass the winter, go through a so-called winter moult before cold weather sets in and this is stated to be not due to any growth-increase, indeed the head capsule of this exuvia is stated to be smaller, not larger, than its predecessor (Burgeff, 1921, quoted by Hering, 1926, pp. 78, 92–93). The moult which the third instar larva of *Rhagoletis pomonella* makes within the puparium to the fourth instar, as described by Snodgrass (1924, pp. 22–23), is likewise unaccompanied by growth. Older individuals of the Cladoceran *Chydorus sphæricus* are stated

by Rammner (1928, p. 297) to undergo several moults without changes of form worth mentioning and without growth; variations in growth-intensity here are thought to be correlated with reproduction.

Growth without Moulting.—That there may be growth without moulting is shown by the observations of Eidmann (1924*a*, p. 126; 1924*b*, p. 585) on *Dixippus morosus* to the effect that after the last moult an increase of weight to about twofold occurs, of course without any resulting ecdysis. This is approximately the increase in weight between the preceding moults. As he expresses it, there thus follows on the last moulting, in a certain measure, a further larval stage which, without change of the skin, passes into the sexually mature condition. He thinks that this probably occurs in all insects with incomplete metamorphosis, and rejects the idea that growth pressure is the immediate cause of ecdysis (1924*a*, p. 127, 1924*b*, p. 589).

Moulting as an Excretory Process.—Another explanation of moulting in Arthropods regards this phenomenon as a mode of excretion and Folsom (1920) writes: "In Collembola ecdysis is something more than a provision for growth, occurring as it does after growth has ceased, and being, in part at least, an excretory process. With each moult the inner half of the epithelium of the mid-intestine is cast off and discharged from the food canal and with it are expelled pseudo-crystals of sodium urate which have previously accumulated in the epithelial cells. This process is correlated with the absence of Malpighian tubes in the Collembola."

Titschack (1926, p. 566) at the conclusion of his detailed studies on growth of the walking stick, *Carausius morosus*, and of the clothes moth, holds that: Moulting is not bound up with growth, is not the consequence of wear and tear, has nothing to do with periodical phenomena, but is to be considered as a metabolic process which contributes to the removal of poisons from the body.

Whether an Arthropod be increasing or decreasing in size, metabolism of course continues and waste materials must be

eliminated. If moulting serves this end, ecdysis will occur whatever be the change, or lack of change, in the volume and in the weight of the body.

Starvation Effects on Insects and on Other Animals.—Recent summaries of growth [7] have noted the effects of starvation on vertebrates and on *Planaria*,[8] but they have suggested no correlation between the results obtained for these animals and those of Wodsedalek and Titschack, to which reference has been made above. The reduction in size in *Planaria*, in *Trogoderma*, in *Tineola* seems comparable, but there is as yet no evidence of the dedifferentiation and rejuvenescence in the second and third of these organisms which have been described for the first, unless the prolongation of their lives be an expression of such conditions.

Growth Factors in General

The abruptness of the change in length which undoubtedly accompanies the moulting of many Arthropods has enabled investigators to detect an approximately constant ratio between the successive stages. Possibly the earliest of these announcements was that by Brooks (1886, pp. 5, 105) who, in describing the early stages of Stomatopod Crustacea, wrote: ". . . the measurements usually enabled me to decide with confidence whether a given larva does or does not belong to a certain series. In a few cases these comparative measurements gave proofs of specific identity which could hardly be made more conclusive by rearing the larvæ. Thus the lengths of the series of *Coronis* larvæ shown in Pl. XIII, Figs. 1–8, are as follows, and if the length of the first stage be successively multiplied by five fourths of itself,[9] and this number by five fourths of itself again, and so on, we obtain the series of numbers given in the second line, and as it is not conceivable that an accidental collection of larvæ should

[7] De Beer, 1924, pp. 89–91. Brody and Hogan in Robbins et al., 1928, pp. 36–37, 98–107.

[8] Child, 1915, pp. 155–175, 300–303.

[9] This repeated phrase, "of itself," appears to be actually subversive of Brooks' meaning, for on p. 105, referring to the same case, he says: the length of the larva increases uniformly at each moult by one fourth of its length before the moult.

exhibit such exact conformity to a numerical law, we may feel certain that these larvæ are genetically related, that they belong to one species or else to closely related species, and that the series is consecutive, with the exception of one missing stage before the last."

The numerical relation thus pointed out was termed "Brooks' law" by Fowler (1909, p. 224), who modified it to read: "During early growth each stage increases at each moult by a fixed percentage of its length, which is approximately constant for the species and sex." Fowler called this percentage the "growth factor." Seymour-Sewell (1912, p. 316), with a knowledge of these papers, gave growth-factors for several marine copepods.

Rammner (1928) studying Cladocera and Gurney (1929) studying Copepoda were impressed by the fluctuations in the intensity of growth, the complete irregularity of the growth factor, the differences shown by different individuals, the occurrence in older Cladocera of moults without change in form worthy of mention and without growth, and arrived at such conclusions as: 'Brooks' law of growth has no value for these Cladocera,' 'it can not be upheld.'

The next attempt to establish growth indices in Arthropods was apparently that of Dyar (1890), who pointed out that "the widths of the head of a [lepidopterous] larva in its successive stages follow a regular geometrical progression." The ratios which he determined, by dividing the width of the head of the last stage into the width of the head of the preceding stage, and so on, were different for the different species and ranged from .58 to .79. Ripley (1924, p. 280) and Imms (1924, p. 183) have termed this progression "Dyar's Law," but the latter, in his example, has given the reciprocal of the ratio used by Dyar, which reciprocal is the Brooks ratio. The Dyarian ratio has been discussed, among recent authors, by Friend (1927, pp. 442–444) for the birch leaf skeletonizer, *Bucculatrix canadensisella*, and by Peterson and Haeussler (1928) for the Oriental peach moth, *Laspeyresia molesta*, who find in the latter species that the average

Dyarian ratio is approximately .59 when there are but four instars and approximately .68 when there are five instars, fewer instars being correlated with more rapid growth. Ripley (*l.c.*) has commented on "The inconstancy of Dyar's supposed constant, . . . varying in *Agrotis ypsilon* from 1.28 to 1.84" (here again the reciprocal of Dyar's ratio). Dyar's ratio has also been determined for the stone fly *Nemoura* by Wu (1923) who finds it to be approximately .95 for 22 larval instars; this is the equivalent of 1.05 of the Brooks ratio.

In 1912, with apparently no knowledge of the growth factors indicated for the Crustacea by Brooks and by Fowler, Przibram and Megusar summarized (p. 712) some of their observations on the Egyptian preying mantis, *Sphodromantis bioculata*, as follows: With each moult the shed skin is double the weight of the preceding exuvia and the weight of the just moulted insect shows a doubling from moult to moult. The increase in length of the prothorax from one moult to another is on an average the cube root of 2, or 1.26, while increase in area is at the rate of the square root of 2 (1.41 +). The biological stages follow a geometrical progression whose exponent is 2, corresponding to the division of the cells. Stzern (1914), in Przibram's laboratory, attempted a demonstration of this last correlation.

It is noteworthy that the ratio suggested by Brooks, viz., 1.25, and by Przibram and Megusar, viz., 1.26, are practically identical, but while Brooks assigned no cause for the value of this growth factor, Przibram sought to connect it with a division of the volume of cells, whose linear dimensions would consequently increase as the cube root of 2.

Yagi (1926) has recently published a mathematical analysis of the body weight and volume and of the weight of the silk glands of the silk worm in the fourth and fifth instars, and of Eidmann's data (1924) on the growth of the walking stick, *Dixippus* (or *Carausius*) *morosus*. He concludes that the growth in an instar of both *Dixippus* and the silk worm takes place in two growth cycles which together constitute the feeding period, and denies Przibram's deductions as to the number of cell divisions in an instar of *Dixippus*.

It is of interest to note in this connection that considerable growth may occur in some animals without any increase in the number of cells. Thus, in the rotifer, *Proales sordida*, the length of the animal at hatching is about 150 micra, at the production of the first egg about 200 micra; during this period which, at a temperature of 20°–21° C., varies from 19.5 to 38 hours, the growth "is entirely by change of form, increase of content of cells and the like, with no increase in the number of cells" (Jennings and Lynch, 1928, pp. 351–352). According to Spemann (1924) this phenomenon was noted in the rotifer *Hydatina senta* by Martini in 1912 and Hirschfelder in 1910.

It should not be overlooked that Przibram and Megusar found and recorded (1912, Tabelle D, p. 723) a considerable range of variations in their weighings and measurements. Thus, the ratios for the weights of the exuviæ ranged from 1.54 to 2.54, mean 2.02, and of the living insects from 1.40 to 3.36, mean 2.09, for the length of the prothorax in the exuviæ from 1.18 to 1.36, mean 1.26, and in the living insects from 1.13 to 1.38, mean 1.29. These figures are for the first five moults; those for moults VI–X (Tabelle E, pp. 724–725) show a still greater range in each of the four series. The authors accordingly concluded (p. 712) that in many cases instead of doubling its weight from one moult to another, the animal more than doubles and this is usually followed by a standstill in the following stage.

The doubling of volume resulting from a single division of the component cells they styled a "division step" (Teilungsschritt). Where the ratio between any two consecutive moults for volume exceeded 2 and that for length exceeded 1.26, they assumed the existence of a sufficient number of division steps between the two moults to preserve the ratios of 2 and of 1.26, as in the silk worm, which, in Europe, usually passes through five moults—an abbreviated number in contrast with the more primitive 9 to 12 moults of the mantis—and to which they assigned 12 division steps (p. 710; also Przibram 1913, p. 83).

This method of explaining the high "growth factors" of

insects with relatively few instars has been employed also by later authors such as Bodenheimer (1927) for Acrididæ, Rhynchota and Holometabola and Alpatov (1929) for *Drosophila*. Ripley (1924, p. 284) takes the view that the largest number of larval stages is the most specialized condition in the Noctuidæ and adds "This conclusion is supported by all the other evidence available." [10]

The computation of "the growth factor" by Brooks and by Przibram and Megusar is by a process similar to that which Brody (1927—bull. 97, p. 22) has termed the "simple interest method of Minot" and which, using Brody's symbols, may be represented by the formula $\frac{W_2}{W_1}$. Brody's actual formula for this method omits unity and has the form $100\frac{W_2 - W_1}{W_1}$. Dyar's ratios are the reciprocals of those obtained by Brooks or Przibram and are $\frac{W_1}{W_2}$. The natural logarithmic

[10] Other length-growth factors per instar have been given for various arthropods as 30 per cent. in Acrididæ (Carothers, 1923, p. 21). For *Dixippus morosus* one third (33⅓ per cent., Eidmann, 1924*a* p. 125; 1924*b*, p. 582) or 25 per cent. (Teissier, 1928*a*), which may be contrasted with the ratios 1.85, 1.54, 1.43, 1.30, 1.33, obtained from the Severins' (1911) rearing of the American phasmid *Diapheromera femorata* ♂. Horsfall (1929) has given the widths of the head capsule in the successive exuviæ of the larva of the grape-vine sawfly (*Erythraspides pygmæus*) from which one may calculate the successive growth-factors as follows: 1.67, 1.50, 1.31, 1.21, 1.16. The data given by Landis (1923) for the four instars of larvæ of *Culex pipiens* show the successive growth factors (total length) as 2.00, 1.38 and 1.69. Similarly Lundblad's (1921) data for *Hydrometra stagnorum* (5 larval stages) yield successive growth factors (body length) of 2.0, 1.5, 1.5 and 1.33. Special interest attaches to the 17-year, or Periodical, Cicada (*Tibicen septendecim*) on account of its long larval life, consisting, however, of only six instars; the growth factors deduced from Marlatt (1907) are 2.22, 1.50–2.00, 1.67–1.87, 1.7–1.13, 1.6♂ or 2.06♀.

According to data by Schreiber (1922, pp. 528–529) the growth ratio for the Ostracod *Cyprinotus incongruens*, stages I–II is 1.32, for stages VIII–IX is 1.59. Those assembled by Herrick (1911, pp. 362–363) and Terao (1928) from Hadley for the lobster, *Homarus americanus*, yield values ranging from 1.18 in the younger to 1.04 in the oldest stages; 35 stages are tabulated.

Interesting are the data given by Mackay (1929) for three lots of postlarval lobsters, each lot kept in an anchored box, all three under similar conditions for a year and for which the growth factors for that period are respectively 1.39, 1.32 and 1.26, the highest rate that of the youngest stage, the lowest rate that of the oldest stage of the experiment.

No attempt has been made, of course, to bring together here more than a few of the date available in taxonomic descriptions of various arthropods.

percentage rates of growth, which Brody (*l.c.*) recommends as truer, may be obtained from the Brooks or Przibram ratios by the following conversion rule: Find the natural logarithm of the growth factor and multiply by 100. Thus, the growth factor 1.26 is, by Minot's method, 26 per cent. The natural logarithm of 1.26 is .231112 which, multiplied by 100, is 23.1112 per cent., or the natural logarithmic percentage rate of growth of Brody.[1]

Precisely the same result is attained by the formula of Schmalhausen (1928, p. 472), viz.:

$$\text{Constant of Volume} = \frac{\log. V_1 - \log. V}{.4343\ (t_1 - t)}$$

or

$$\text{Constant of Length} = \frac{\log. L_1 - \log. L}{.4343\ (t_1 - t)}$$

as $(t_1 - t)$ usually equals unity.

Growth Factors of Odonate Larvæ

With the background afforded by the preceding discussion of Growth Factors in general, we may examine Tables 3 and 4 which give the available data for several species of Odonata. If an arthropod grows at such a rate that each instar becomes 1.26 times the length of the preceding instar, its length in the second instar will be n (the initial length) $\times$ 1.26 (the first power of 1.26), in the third instar $n \times 1.26 \times 1.26$ or $n \times 1.26^2$, and so on. If the growth continue at this rate, then y (the length of instar x) will equal $n \times 1.26^{x-1}$ and the ratio between n and y will be $1 : 1.26^{x-1}$. If we compare the powers of 1.26 given in Table 5, with the ratios A to B in Table 3, we see that the second member of the ratio 1 : 9 for *Lestes viridis* falls between the 9th and 10th powers of 1.26, the former (9th) being the same number as the total number of larval instars recorded for that species; the formula $\frac{n}{y} = \frac{1}{1.26^{x-1}}$ therefore does not apply, as the larvæ grew more rapidly than

[1] In the absence of a table of natural logarithms, such may be calculated by dividing the common logarithm of a desired number by .4343.

TABLE 3

ODONATA REARED FROM THE EGG

Name and Number Reared	Place Where Reared	Initial Larval Length in Mm. (A)	Maximum Larval Length in Mm. (B)	Ratio of A to B	Total Number of Larval Instars	Duration of Larval Life in Days	Source of Information
LESTINÆ							
Lestes viridis (6)........	Kufstein, Tirol	3.0[1]	27.00[1]	1 : 9	9	60–65	Prenn, 1926
Sympycna pædisca (2?)...	Kufstein, Tirol	2.5[1]	27.0[1]	1 : 10.8	10	72	Prenn, 1928
AGRIONINÆ (Selys)							
Argia extranea 1.........	Cartago, Costa Rica					210	Calvert, 1910, 1917
Agrion pulchellum 7......	Sutton, England and Larne, Ireland	1.3[2]	14.–22.[2] (aver. 18)	1 : 10.8–16.9 (1 : 13.8)	10–13	229–634	Balfour-Browne, 1909
Ischnura elegans 1.......	Do.				12		Do.
Pyrrhosoma nymphula....	Göttingen, Germany	1.2[2]	12.–15.2[2] (aver. 13.5)	1 : 10–12.67 (1 : 11.25)	9	ca. 315	Backhoff 1910
AESHNINÆ							
Anax imperator (3)......	Basel, Switzerland.	2.	47.	1 : 23.5	12	195–300	Portmann 1921
Anax junius 1...........	Cheyney, Penna.	2.12	44.	1 : 20.75	13	322–328	(This paper)
LIBELLULINÆ							
Libellula depressa........	Basel, Switzerland.	1.5	22.5	1 : 15	12	365–730	Portmann 1921
Pantala flavescens........	Honolulu	1.[3]	24.	1 : 24	11[3]	55–101	Warren 1915
Pantala flavescens (1–4)..	Phila., Penna.	1.38	24.87[4]	1 : 18		(80[5])	Lamb 1925
Sympetrum vicinum (4)...	Phila., Penna.	1.08	12.48–14.24	1 : 11.5–13.2	11	97–101	Nevin, 1929
Nannothemis bella (4)....	Cheyney, Penna.	0.8	9.25–9.69	1 : 11.9–12.1	11–12	674–1037	(This paper)

[1] Including the caudal gills.
[2] Excluding the caudal gills.
[3] First instar given is evidently the pronymphal stage, hence his "12th instar of nymph" is the 11th larval stage, cf. Warren p. 8.
[4] Last larval exuvia.
[5] To end of 10th instar only. Compare Miss Lamb's later paper of 1929.

TABLE 4

ODONATE LARVÆ—PERCENTAGE OF INCREASE ("GROWTH FACTOR") FROM INSTAR TO INSTAR

The second horizontal line of figures for each species gives the number of days in each instar. Sources of information the same as in Table 3.

Species	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII	XIII
Lestes viridis........	1.5?	1.17	1.57	1.27	1.29	1.56	1.36	1.26	1.12				
	6	5	5	6	3	5	14	11	7–12				
Sympycna pædisca...	1.47	1.60–1.20	1.67–1.25	1.60–1.40	1.43–1.25	1.10	1.18	1.38–1.23	1.5–1.22	1.23–1.12			
	9	3	3	6	5	4	4	7	14	17			
Agrion pulchellum...		1.22	1.31	1.27	1.37	1.26	1.26	1.33	1.14	1.21			
	7–12	14–24	13–55	7–168	4–150	9–142	6–51	5–46	9–94	17–234	26–227	37–71	211–217
Pyrrhosoma nymphula		2.25	1.56	1.33	1.25								
Anax imperator......		1.25	1.20	1.83	1.27	1.29	1.44	1.31	1.35	1.26	1.14	1.42	
	3	5	3	5	7	8	18	12	18	26	45	38–44	
Anax junius........	1.30	1.16	1.18	1.10	2.00	1.38	1.38	1.29	1.25	1.31	1.29	1.16	1.07
	4–13	13–17	14–16	2–4	11–12	2–4	5–11	14–18	126–134	23–28	26–27	15–23	42–49
Libellula depressa....		1.33	1.25	1.20	1.50	1.22	1.45	1.19	1.24	1.32	1.26	1.15	
Pantala flavescens (Honolulu).......		1.50	1.55	1.72	1.25	1.20	1.33	1.25	1.30	1.38	1.33		
	4	4.5	4.5	5	4	4.75	4.75	5.75	5.5	11.67	26.3		
Pantala flavescens (Philadelphia).....		1.30	1.22	1.29	1.38	1.25	1.50	1.11	1.31	1.31			
	6	6	6	7	7	7	7	9	11	14	11+		
Sympetrum vicinum..		1.28	1.19	1.30	1.29	1.35	1.33	1.30	1.33	1.35	1.25		
		3–7	11–14	2–9	2–10	6–7	7–11	10–14	7–14	10–11	25–29		
Nannothemis bella....	1.19	1.17	1.22	1.30	1.26	1.20	1.22	1.33	1.34	1.31	1.27	1.22	
	12–14	21–23	7–8	12–17	21–24	14–194	7–50	15–63	35–266	6–71	32–337	ca. 272	

the calculated rate, as may be seen also from an inspection of Table 4 where the growth factors exceeded 1.26 in five of the

TABLE 5

POWERS OF 1.26

1, 1.26	3, 2.00	5, 3.17	7, 5.03	9, 7.99	11, 12.69	13, 20.15
2, 1.59	4, 2.52	6, 3.99	8, 6.34	10, 10.07	12, 15.99	14, 25.39

eight instars, II–IX. Similarly, this formula does not apply to the other species except *Nannothemis bella* with twelve instars. All of the other nine species grew at a more rapid rate than the 1.26 growth factor requires and if one substitute the natural logarithmic percentage rate, or 1.23, the difference will be still more marked.

On inspecting Table 4, it will be seen that the growth factor, calculated from the existing data, often varied greatly from instar to instar. These data, in most cases, do not allow us to do more than assume very varying rates of growth. One instance, however, is interesting and worthy of mention here. The third, fourth and fifth exuviæ of the reared specimen of *Anax junius* which represents this species in Tables 3 and 4, measured 3.35, 3.68, and 7.36 mm. respectively, giving the growth factors 1.18, 1.10 and 2.00 enteıed in Table 4, and the dates of these moults were September 5–6, September 8–9, and September 20, all in 1926. Among material collected at Glen Mills, Pennsylvania, (two miles distant from Cheyney) on September 5, 1927, was an exuvia 5.81 mm. in length which appears to correspond morphologically to a fourth exuvia. If it were substituted for the fourth in the above series, the growth factors would be 1.18, 1.73 and 1.27; in any event it represents a different rate of growth from that of the larva recorded in Table 4.

Finally Tables 6 and 7 give the data for each of the four reared larvæ of *Nannothemis bella* the averages of which have been entered in Tables 3 and 4.

In 1920, E. Fischer observed that the length of the caterpillars of five European species of *Argynnis*, measured

at their successive times of moulting, corresponded closely to the so-called Fibonacci's [1] Series, that is, each member of the series, beginning with the third, was equal to the sum of the preceding two members. Thus, in *A. latonia*, which moults four times, the successive lengths are 3, 4.5, 7.5 and 12 mm.; in *A. laodice*, which moults five times and including the length of the last larval instar just before pupation, the observed data are 3, 4 (4.5), 7 (7.5), 10.5, 18 and 28–32 mm. No such mathematical series is to be found in the lengths of successive instars of Odonata.

TABLE 6

NANNOTHEMIS BELLA. NUMBER OF DAYS IN EACH INSTAR

	I	II	III	IV	V	VI	VII
No. 58	12	23	8	12	21	187–194	50–36
No. 65	12–13	23	7	14	207–214	28–17	7–14
No. 66	14	21	8	14–17	203–213	38–46	33–25
No. 56	12–13	22–23	8	12	208–201	14–35	28–14
Range	12–14	21–23	7–8	12–17	21–214	14–194	7–50

	VIII	IX	X	XI	XII	Total
No. 58	35–49	85–71		250–243	0	674
No. 65	28–15	35–49	21–6	32–46	276–269	686
No. 66	49–56	224				
No. 56	49–63	266–252	59–71	337–332	0	1037
Range	15–63	35–266	6–7	32–337	ca. 272	674–1037

TABLE 7

NANNOTHEMIS BELLA—GROWTH FACTOR FOR EACH EXUVIA

	I	II	III	IV	V	VI	VII	VIII	IX	X	XI	XII
No. 58	1.22	1.27	1.02	1.28	1.44	1.11	1.22	1.21	1.36	1.36	1.37	
No. 65	1.12	1.09	1.34	1.37	1.18	1.19	1.26	1.23	1.12	1.26	1.26	1.22
No. 66	1.12	1.16	1.31	1.24	1.24	1.30	1.20	1.73				
No. 56	1.32	1.16	1.53		1.18	1.22	1.21	1.15	1.54	1.31	1.18	
Average	1.19	1.17	1.22	1.30	1.26	1.20	1.22	1.33	1.34	1.31	1.27	1.22

[1] So named from Leonardo of Pisa (Leonardus Pisanus or Fibonacci), Italian mathematician of the thirteenth century.

Comparison of Arthropod and Vertebrate Growth Factors

It will also be noticed that the growth factor does not exhibit either a continuous increase or a continuous decrease from the first to the last larval exuvia in Tables 3, 4 and 7. Other arthropods have been recorded[13] as showing the same characteristics. These facts would seem to detract from the value of the comparison of growth in warm-blooded and in cold-blooded animals (Brody, 1927, Bull. 98). According to this author (*l.c.* and Bull. 97, pp. 68–69), the period of growth may be divided into two phases, the self-accelerating and the self-inhibiting. The age of puberty coincides with the age of junction between the two phases. The growth made by the pre-adult stages of arthropods can be compared, therefore, only with the self-accelerating phase. This phase in warm blooded animals "is (1) made up of several (4 or 5) segments apparently representing distinct stages of growth; (2) the percentage rate of growth during each of these stages is constant; (3) each stage of constant growth rate passes into a succeeding stage of a lower, but constant percentage-rate; (4) the passage from one stage to the other is abrupt, the abruptness being of the order found in metamorphosis in cold-blooded animals" (Bull. 98, p. 5). Thus the successive logarithmic percentage rates of growth *in weight* for the albino rat are given as 53 (prenatal), 12, 4.5 and 3 (postnatal) per day.[14]

MacDowell (1928) has pointed out that the changes in rate of postnatal growth of the mouse coincide with changes in the character of its food, as at birth and at weaning. The changes in growth rate indicated by the difference in growth factors of the successive instars of Arthropods can not, in many cases, be associated with changes in food. Their

[13] *E.g.*, Various Crustacea (Gurney, 1929, pp. 204–206), Cyclopidæ (Ziegelmayer, 1925, Tabellen 8, 16, 17), the mantid *Sphodromantis bioculata* (Stzern, 1914, p. 481 and Tabellen, XI–XV), honey bee larvæ (Nelson, Sturtevant and Lineburg 1924, pp. 15, 19), the clothes moth larva, *Tineola bisselliella* (Titschack, 1926, pp. 526–538).

[14] It must not be overlooked that the Brooks-Przibram ratios given in this paper, or quoted from other authors, for Arthropods are in almost every case for periods longer than one day.

successive moults afford convenient and "natural" "milestones" for estimating growth rate, but the very detailed records of growth, such as those published by Titschack for *Carausius* (*Dixippus*) *morosus* and *Tineola biselliella*, and even the less complete data contained in this present paper for *Anax junius* and *Nannothemis bella*, show clearly that, even within the same instar, the rate varies considerably.

The Elimination of the Time and Stage Factors in Estimating Rates of Growth

Most of the determinations of rates of growth have had reference to units of time (days, months, weeks, or years), or to stages demarcated by moults. Temperature, by increasing or decreasing the rate of growth, renders the time units so variable that they are often useless in comparing the growth of different species or individuals. Food or its absence, humidity and, in some cases, temperature affect the number of moults, as has been shown *anteà*, so that these latter lose much of their significance as indicating definite points in growth. My colleague, Doctor R. L. King, has therefore suggested the advisability of adopting other coördinates in constructing curves of growth, namely, the plotting of definite fractions of the total growth against definite fractions of the time consumed in making that growth. Figs. 2, and 3, embody this principle and afford what should be a more equal basis of comparison of the growth rates of different species and individuals.[15] Even this method, however, does not eliminate some of the effects of presumably winter temperatures, as may be seen by comparing the curves of *Nannothemis bella* and *Anax junius* in Figs. 2 and 1.

Relative Sizes of Adults and Preadults among Animals

One of the most conspicuous and familiar characteristics of the ontogeny of winged insects is the difference in volume

[15] For the data on which these curves of two species of grasshoppers are based, I am indebted to another colleague, Doctor J. H. Bodine, who made the actual observations. I find that Saller (1927, pp. 544–549), if not others, had already used this method of constructing growth curves.

and in form of the body which so often exists between the larva and the imago. In those with a complete transformation the body length of the adult is frequently and markedly less than that of caterpillar, grub or maggot; few

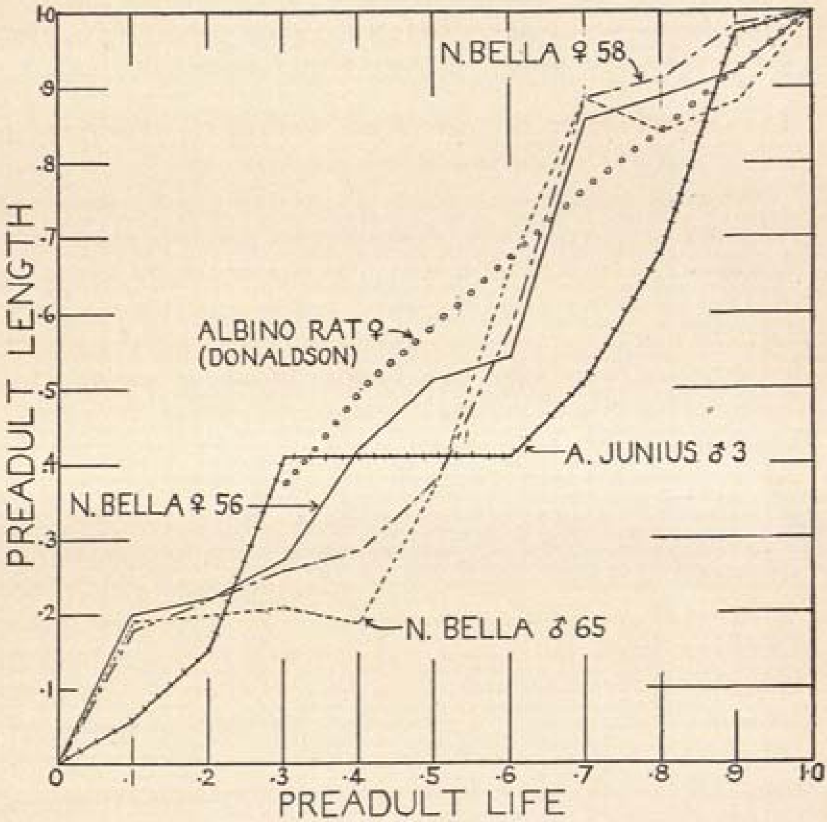


Fig. 2.

such striking repressions of linear growth during the reproductive period are to be found elsewhere in the animal kingdom, but such cases appear to be presented by the brook lamprey, *Entosphenus wilderi* (Gage), (Okkelberg, 1921), the lung-breathing adult of the axolotl and some toads.[16]

[16] Boulenger, 1897, pp. 109–110, mentions five species of European Anura in which the maximum length of the tadpole is greater than that of the adult and two species in which the lengths in the two stages are nearly the same.

On the other hand the imagos of insects which pass through an incomplete metamorphosis usually exceed in length the larvæ from which they have developed, and in this they agree with the non-insectan arthropods and practically all other animals. In the tornaria larva of *Balanoglossus*, in the "tadpole" larvæ of some Ascidians and frogs, in the Leptocephalus larva of certain eels, there is a decided shrinkage in

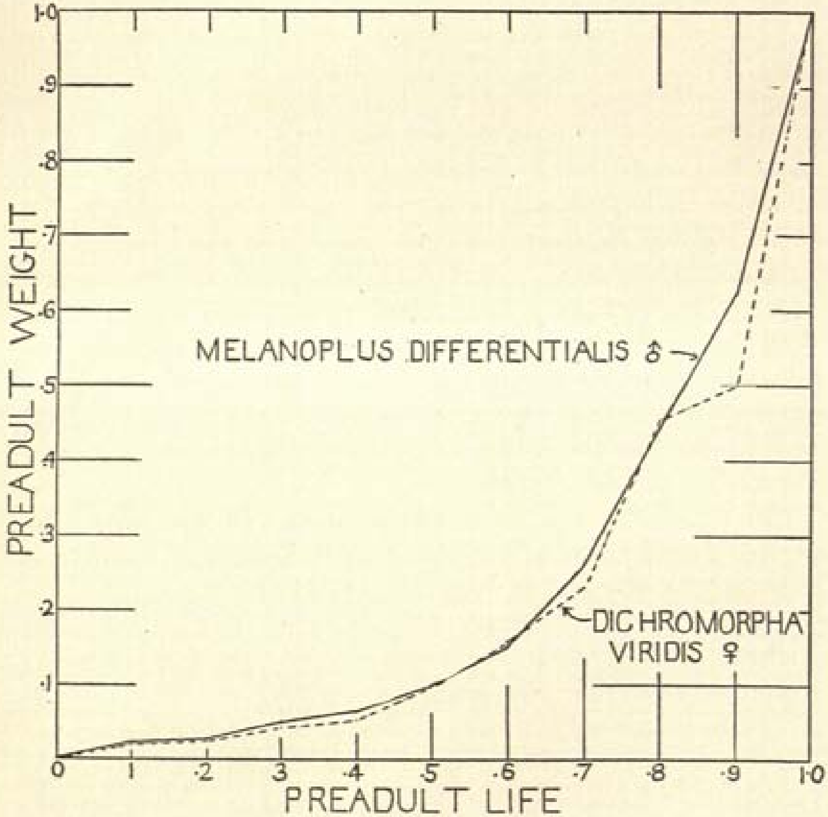


Fig. 3.

Fig. 3.—*Melanoplus differentialis*, male, fed on grass at 25° C., December 15 to March 20, = 92 days. *Dichromorpha viridis*, female, fed on lettuce at 25° C., December 20 to February 28, = 70 days. Both species are grasshoppers (*Acrididae*). Data by J. H. Bodine.

length, if not in other dimensions, at or immediately following metamorphosis, but in these the adult usually, if not always,

surpasses its larval size.[1] Yet it is in the heterometabolic insects that the discontinuous, step-like character of the length-growth curve is more marked and this has been correlated with the firmer chitin of their larvæ. It is in the larvæ, with softer, more pliable chitin, of the holometabolic insects (excluding Coleoptera) that growth tends to be more continuous and hence to approach, in this characteristic, the other metazoan phyla.

All animals probably increase in weight at, or immediately after, sexual maturity. This is partly due to the increased weight of the reproductive organs, partly to that of the somatic tissues. In insects and many arachnids the period of reproduction is usually shorter than the prereproductive life of the same individual, is confined to a limited season and is soon followed by death; no increase in the soma (weight or volume) is apparent. Stated in another way, "puberty" occurs at a relatively late point in the total span of life. Well-known exceptions occur, however, in the reproductive females of the honey-bee and other over-wintering Hymenoptera, in Chalcids, termites, some mosquitoes and *Drosophila*, both in the length of the reproductive period and in increase of the somatic tissues.

In Crustacea and in many members of other phyla the period of sexual maturity is relatively prolonged, the ability to reproduce is repeated at intervals, and the tissues not directly concerned in this function may become both heavier and larger. In such cases the beginning of sexual maturity is shifted to an earlier biologic age.

A few words may be added here on the *relation between the length of life and the final size of the individual.* Hering (1926, p. 260) credits Standfuss with the discovery of a "law" which states that the size of the butterfly or moth increases with the length of the developmental period and Szabó (1927) has cited representatives of many classes of Metazoa in support of the statement (p. 53) that the larger species are the longer-

[1] Harmer, 1910, p. 21, Delage et Herouard, 1898, p. 61, Boulenger, 1897, pp. 109–110, Wright, 1914, p. 21 and Dickerson, 1908, for the species listed by Wright, *l.c.* Special cases like the medusæ which arise from hydroid colonies are excluded from the comparisons made above in the text.

lived. Saller (1927, p. 538), discussing rodents, asserts that "Duration of growth and final size are determined by factors characteristic of the species which are independent of each other . . . within the species a certain relation can exist between duration of growth and final size, but not necessarily so." The data given in our Table 3, although for the preadult stages only, and especially our comparisons between *Nannothemis bella* and *Anax junius*, afford no ground for affirming such a relation in Odonata; even if we had exact figures on the length of the life of the imagos, there is no reason to think that they would establish this relation.

The Supposedly Universal Characteristic of Growth Rate in Animals

Numerous investigators of growth rates have been impressed with the very great irregularity of growth of the individual.[1] Nevertheless there is a characteristic of post-embryonic growth rate which apparently holds true for many animals, namely, that it is highest, with respect to the size of the individual, when the organism is still very young; in other words, the percentile rate is greatest in early age.[2] The few data which exist on embryonic rate seem to indicate that for that period also the rate is highest at its beginning.[3] This harmonizes with Conklin's statement (1912, p. 185) that the rate of cell division is slower and the resting periods longer in mature animals than in young ones.

In the honey bee larva, however, according to Nelson,

[1] Mead, 1900, p. 23, starfish. Orton, 1924, Patella. Rammner, 1928, p. 597, Cladocera. Gurney, 1929, p. 504, Ostracoda and Copepoda. Ziegelmayer, 1925, pp. 545–556, 566–568, Cyclopidæ. Herrick, 1896, pp. 96–97, 223; 1911, p. 360, the American lobster. Stzern, 1914, p. 481, nos. 17, 18, *Sphodromantis bioculata*. Titschack, 1924, pp. 438–444, *Carausius morosus* (walking stick). Wodsedalek, 1912, p. 371, *Trogoderma tarsale* (museum beetle). Nelson, Sturtevant and Lineburg, 1924, p. 19, honey bee larvæ. Parker, 1926, p. 43, and 1929, turtles and alligators. Saller, 1927, p. 536, rodents. Minot, 1891, p. 134, guinea pig. Hanson and Heys, 1927, albino rats. Also the data on *Nannothemis bella* given *anteà*, pp. 231–2, 243.

[2] de Beer, 1924, p. 80, Brody, 1927, Bull. 98, p. 6, and Child, 1915, p. 276, animals in general. Titschack, 1924, p. 437, *Carausius morosus*, and 1926, pp. 527, 534–538, the clothes moth caterpillar. Pütter, 1920, pp. 328–330, herring and sole. Scofield, 1928, p. 34, striped bass. Schmalhausen, 1928, p. 516, vertebrates. Minot, 1891, p. 148, guinea pig.

[3] Murray, 1925, for chicken embryos.

Sturtevant and Lineburg,[1] "The increase in growth, measured by percentages, tends to attain a maximum at the close of the *second* day and from that time steadily declines until the close of larval development." Fig. 4, lower half, shows the percentile rate for *Anax junius* larva no. 3. It is, of course, based on but a single individual but, even after allowing for

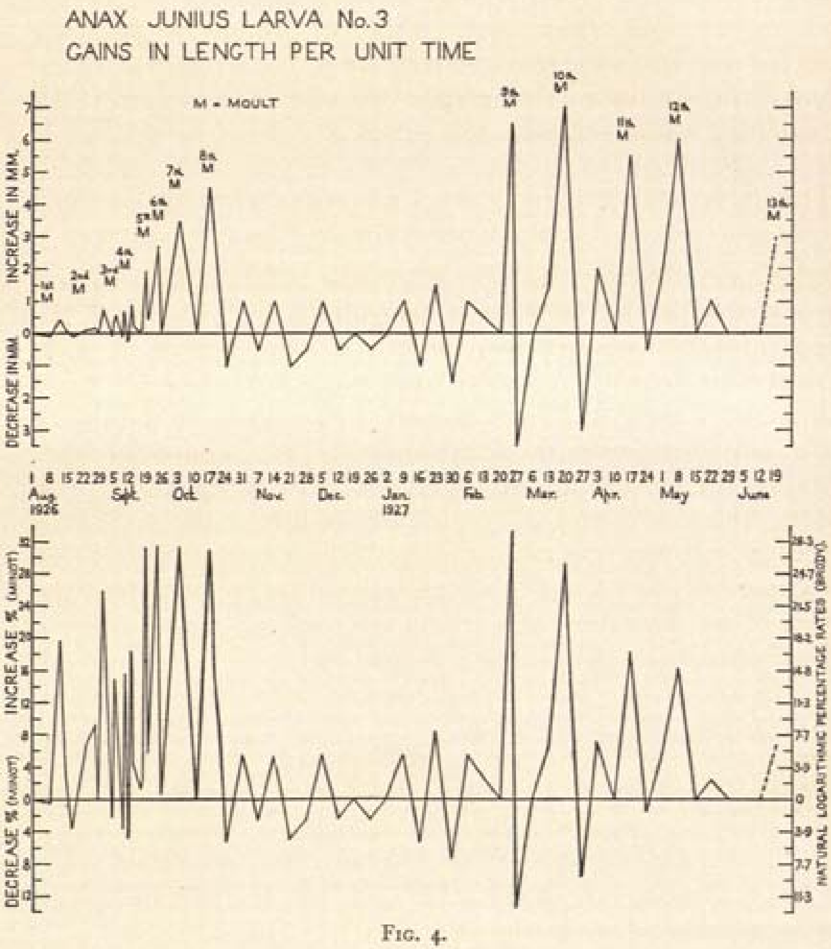
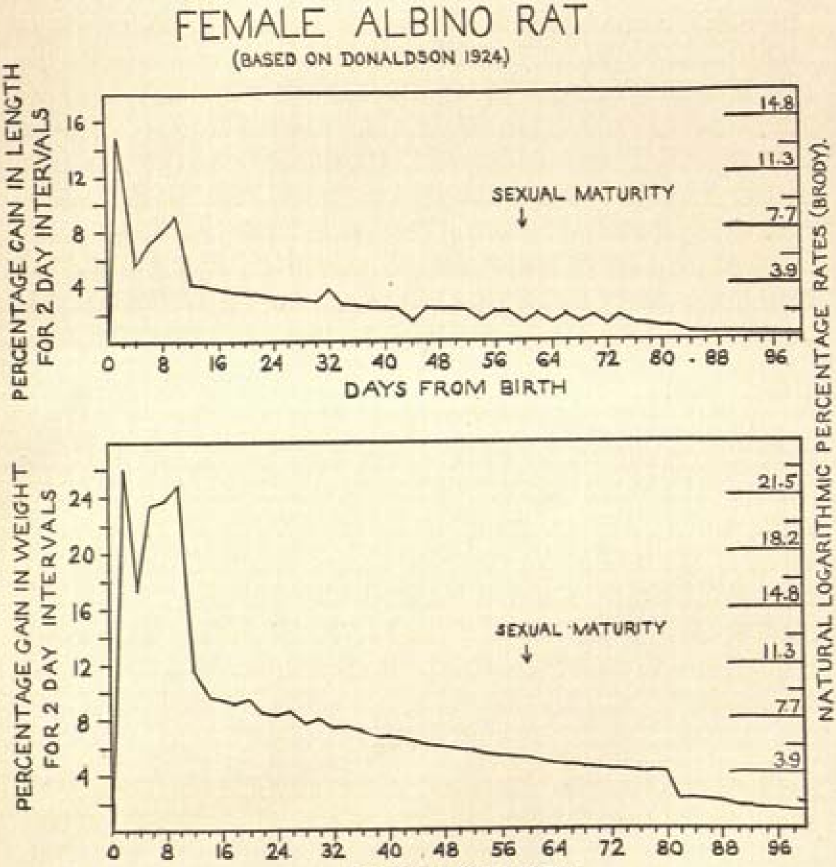


FIG. 4.

individual peculiarities, the very high rate at the age of 30 weeks (9th moult), to say nothing of the rates at the 3d, 5th, 6th, 7th and 8th moults, presents a strong contrast to Fig. 5,

[1] 1924 p. 19; *cf.* their fig. 5, p. 15. The italics in the quotation are mine.

of the percentile rates of growth in both length and weight of the albino rat. Data for constructing comparable percentile curves for *Nannothemis bella* in its earlier instars are not at hand, and the percentile rate of growth in arthropods deserves detailed investigation. Titschack's (1924, p. 449) curve for *Carausius morosus* is of interest in this connection.



Specific Rates of Growth

Thompson asserted that "Every growing organism and every part of such a growing organism, has its own specific rate of growth, referred to a particular direction." (1917, p. 54).

Saller (1927, p. 534) wrote that: "Growth, during the progressive phase and within a time characteristic for each species, results in a body size which is characteristic for that species, which is equivalent to saying that the process moves with a characteristically specific velocity." Gurney (1929, pp. 205–206) thinks that: "Within rather wide limits there probably is a specific growth factor, but there is much individual variation and the factor changes from moult to moult in most cases." With the qualifications expressed by this last author, an ideal specific growth rate may have its advantages as a standard from which the deviations of a given individual may be estimated. Particularly is this the case where, as in the Przibram growth factor, the mathematical ratio refers to a fundamental process like the doubling of cells and the increase of a linear dimension as the cube root of the doubling. We can at least perceive that the individual has grown more or less rapidly than the theoretical consideration requires.

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Summary

Two species of Odonata, *Nannothemis bella* and *Anax junius* were reared indoors, from egg to adult, under similar conditions except that the smaller species (*bella*) was given smaller animals for food. Their growth, food supply and transformation are described, and illustrated by tables and graphs.

N. bella (three individuals) completed the maximum larval length of 9.25–9.65 mm. in 674–1037 days (add 18 days

for the embryonic period); no correlations in size, length of larval life or number of ecdyses appear.

A. junius (one individual) attained its maximum larval length of 44 mm. in 322–328 days (341 days from time of oviposition).

Although the room temperature in which the larvæ were kept was never lower than 10° C., all of them showed a halt in growth in the winters.

Growth in length occurs between moults in *N. bella*, in various Heteropterous, Coleopterous, Lepidopterous and Dipterous larvæ and hence is continuous, as in non-Insectan Arthropods and many other Metazoa. In *Anax junius* and in most heterometabolic insects it occurs chiefly at the ecdyses and is therefore discontinuous. Temporary fluctuations in length occur in representatives of both sorts of insects as the result of ingestion and egestion.

Evidence is given as to the variability in number of moults of the same species of insect, leading to the conclusion that moults are not absolute indicators of biologic age.

The growth factors announced by Brooks, Dyar, Przibram and Megusar and Brody are compared and the Brooks-Przibram ratio given (Table 4) for each instar of ten species of Odonata which have been reared from egg to adult.

Nine of these species grew at a more rapid rate than this ratio demands.

A Fibonacci series of lengths of successive stages of certain Lepidoptera as observed by Fischer has not been found for Odonata.

The growth factor varies in an irregular way from instar to instar in the Odonate larvæ and in other Arthropoda and these variations are not correlated with changes in the character of the food, facts which detract from the value of Brody's comparison of growth in warm-blooded and cold-blooded animals.

To make more exact comparisons of the growth rate of different species and individuals, sample curves are given in which tenths of growth in length to the end of larval life are plotted against tenths of the time consumed.

Adults of insects with complete metamorphosis are usually smaller than their corresponding larval stages; a few non-arthropods show the same character.

Adults of insects with incomplete metamorphosis usually exceed in length the larvæ from which they have developed and in this agree with the great majority of non-insectan animals.

In most insects and in many Arachnids, puberty occurs at a relative late point in the total span of individual life, and the reproductive period is confined to a limited season. In most other animals the period of sexual maturity begins at an earlier biologic age, is relatively prolonged and the ability to reproduce is repeated at intervals.

No relation between length of life and final size has been detected among the Odonata.

Anax junius, in common with some other insect larvæ, departs from the general rule that the percentile rate of growth is greatest in early age.

The idea of a theoretical rate of growth may have some advantage as a standard from which to estimate the deviations of a given individual.

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ON THE NATURE OF THOUGHT AND ITS LIMITATION

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THE within essay has been prompted by the new conceptions which have of recent years arisen concerning the structure of the universe. Not so long ago the atom of the older physicists and chemists was resolved into two portions, one a central portion or nucleus and the other a peripheral portion revolving about the nucleus and relatively distant from it. To the nucleus was assigned the property of a positive electric charge. In the atom of hydrogen, the nucleus was revealed as a single entity which was termed a proton, but in the atoms of all the other elements the nucleus was revealed as being made up of both electro-positive and electro-negative parts but the electro-positive predominating, the nucleus always reacted as a positive entity. To the second or peripheral portion, the term electron or electrons, according to their number, was applied. To them was assigned an electro-negative rôle. The relation between the nucleus and the electrons was comparable in a general sense to the relations which obtain between the sun and the planets of our solar system. These relations were comparable not only to the motions of the electrons about the nucleus, but to the relative distances of the nucleus and electrons from each other. A convenient illustration was presented by the atom of hydrogen which was made up of a nucleus (one proton) and one electron, the latter revolving about the former in the course of an ellipse. In a steadily increasing gradation, a mathematical progression, the number of electrons revolving about the nucleus in the various elements grew progressively larger until finally to uranium 92 electrons were assigned.

This interpretation of the structure of the atom was so simple and so clear that adequate mental pictures could readily be formed. For the time being, these conceptions so beauti-

ful and apparently so satisfactory, were gladly accepted, but unfortunately for our intellectual contentment, they were destined to be rudely disturbed. Two other factors have been added or rather, shall I say, subsequently made their appearance—indeed, have forced themselves upon the attention of the physicist. One of these is the "quantum" by which is meant the energy given out at intervals by an atom, or, if preferred, by an atomic structure or combination. The second is that of the "wave" by which is meant a wave motion which is either intimately associated with the electron or replaces the electron; in fact, itself constitutes the electron.

The question at once arises, what becomes of the mental pictures of the atom we had previously formed? Evidently a stage has been reached at which these mental pictures, alike simple and clear, can no longer be retained, or at least, must be greatly modified or perhaps abandoned. On the one hand, we are confronted by the difficulty of forming a picture having the semblance of reality and, on the other, by the danger of falling into the maze of metaphysical speculation.

Before attempting to form adequate or even approximate mental pictures, let us pause to consider some of the biological facts underlying our mental processes. These it is necessary to present in their special relation to the problem before us. To begin our study a few elementary considerations are indispensable, but simple as these are they are of profound significance.

We do not know, of course, the origin of living matter. Two explanations present themselves; first, that living matter is the result of an act of special creation; or, secondly, that it is the result of the action of previously existing natural laws. The hypothesis adopted by some of the thinkers of a past generation that life is of extra-terrestrial origin, that it had existed in interplanetary space and in some way reached and inoculated the earth, no longer finds adherents. On the other hand, the view that living matter made its appearance upon the earth as a result of the interaction, combination and association of previously existing substances, seems inescapable.

The changes taking place were doubtless very gradual and extended over vast periods of time. The substances formed were probably of increasing complexity and resulted from the synthesis of simpler substances. In an increasing degree, they gradually developed properties which we associate with living matter. A. B. Macallum has advanced the view that life originated in the ocean water of the Archean period and that the very earliest organisms must have been micellar or ultramicroscopic in character. Subsequently aggregates of these micellar particles were formed. These aggregates, at first unnucleated and later nucleated, approximated in complexity the simplest organisms of our own day. It is reasonable to suppose that in some such way, living matter, protoplasm, made its appearance.

We should bear in mind that living matter, as we know it to-day, is not in the ordinary sense, a chemical compound. It is rather an aggregate of many chemical substances; proteins, fats, carbohydrates and electrolytes. In this aggregate the different chemical bodies are so combined that they have for the most part lost their separate identities. The proteins consist of associations or combinations of many amino-acids. The fats and carbohydrates are diffused or suspended in the general protoplasmic mass. The electrolytes are diffused in solution through the protoplasmic mass and are resolved into their constituent ions. The physical condition of protoplasm is that of a colloid consisting of many aggregates which play the rôles of continuous and discontinuous phases. It is not necessary here to dwell upon the fact of the incessant chemical changes which take place in living protoplasm; upon the changes which take place in the proteins, the fats and carbohydrates, changes which are the result in part of the action of catalysts and in part of oxidation. Neither is it necessary to dwell upon the fact that these processes result both in constructive or up-building changes and in reducing or downward changes; nor on the fact, that these changes, incessant and complex, are the features which distinguish, in a large degree, living from non-living matter. Further, we are concerned not

only with the materials, solid, liquid and gaseous, with which this remarkable substance, protoplasm comes into relation in the outside world, materials which serve the purpose of food or make possible those physical and chemical changes upon which its continuance depends, but especially with the behavior of this substance in response to the incident forces of the environment. It is this last factor which in our present inquiry especially and insistently demands our attention.

The first and most primitive impression from the environment to which living protoplasm is exposed is that of contact with foreign bodies. Such contact constitutes, in a sense, a blow, or better, an impact. In such an impact something is developed and transmitted through the protoplasmic mass. This can readily be observed in the amœba and in others of the protozoa; in these organisms the effect of the impact appears to be slowly diffused. In syncytic and simple metazoic forms, such as sponges, the taking up of and transmission of motion can likewise be observed. In sponges the effect of an impact such as the prick of a pin is clearly transmitted, though very slowly. As we ascend the biological scale, we note first in cœlenterates, and, later, in a progressive degree, in higher forms, the differentiation of special pathways for the transmission of the effects of the impacts. Without pausing at this time to consider these, we note that in addition to the impacts of contact, the organism is exposed to impacts that result from the movements and coarse vibrations of the surrounding medium, to impacts that are chemical, and, finally, to impacts that result from forces that pervade the external world.

Second, therefore, in the series of impacts are those derived from the movements and coarse vibrations of the medium in which the organism is immersed. It is not necessary here to speak of the vesicles which are evolved in both invertebrates and vertebrates, small cavities which contain one or more particles of solid mineral matter and by means of which the movements, the coarse vibrations of the surrounding medium are taken up, arrested as it were, and forcibly transmitted

to the protoplasmic mass. At times, such particles of solid mineral matter serve also to influence the relations of the organism to gravitation. The point of importance to be especially noted, is the existence of a mechanical contrivance by means of which gross physical relations of the environment are converted into physical impacts. Some of these evoke in ourselves the sensation of sound. That the mineral particles besides serving as otoliths may also serve the purpose of statoliths and thus give rise to a more or less differentiated gravitational or equilibratory sense need not here be discussed.

Third in the series of impacts are those that are chemical. These, it would appear, have a play within comparatively narrow limits. If living protoplasm is exposed to the gross action of an acid or an alkali, its destruction necessarily follows. There is, however, a wide intermediate range in which chemical action can and does take place without such result; quite commonly such impacts are almost inconceivably minute in character, and without pausing to consider their action upon the protoplasmic mass as a whole, we note especially that their action in the higher organisms is accompanied by the sensations of smell and taste.

Fourth in the series of impacts are those that result from forces that pervade the external world. The first of these to engage our attention is light. At once we note that the simpler forms of life, for example, the amœba and other protozoa are largely transparent to light; light on the whole is freely transmitted by them. However, we note in some protozoa the appearance of a small mass of red or dark red pigment, a so-called eye spot or stigma. This mass is clearly different in its reaction to light from the surrounding protoplasm. Its action is evidently to arrest or transform the light vibrations and apparently to transmit this transformed energy to the general protoplasmic mass. Here we have a second instance in which a special device is evolved to arrest impacts received from the external world.

Light falling upon or passing through living protoplasm

greatly influences, of course, its chemical changes, but with these we are not here directly concerned. Similar is it with regard to the second and allied force to engage our attention; namely, heat. The reactions of rhizopoda and other protozoa to variations of temperature are, in a general way, known. These are, of course, very general in character. Special contrivances for the "arrest" or "taking up" of heat rays do not appear to have been evolved until relatively late in the history of the metazoa. However, regarding their actual existence in ourselves, our ability to appreciate hot and cold and indeed many gradations of temperature offers indisputable proof.

As a result of the above considerations, we are confronted by two striking and outstanding facts: first, by the presence of special arrangements or devices for arresting impacts received from the outside world, and secondly by the fact that these arrangements are exceedingly small in number. Further, the impacts are limited as follows: First to those of contact, received by the general surface in protozoa or by special structures termed receptors in metazoa; secondly, to those the result of movements or coarse vibrations in the surrounding medium, giving rise to sound, to static and to equilibratory reactions; thirdly, to those resulting from chemical conditions or changes in the surrounding medium, and fourthly, to those resulting from light and, in given instances, from heat vibrations. These are the only impacts which according to the facts before us living protoplasm is capable of receiving; these are the only impacts which directly affect it. This is true apparently of all of the higher metazoa; demonstrably is this true of ourselves.

Another outstanding fact remains, and this is that for other impacts than those just enumerated, the organism is without any provision for their reception. Vast ranges of other agencies exist in nature but concerning them we have only an inferential knowledge. Besides contact, touch will give us information only of coarse vibrations numbering less than 30 per second; thence, vibrations from 30 to 30,000 per second are appre-

ciated by the ear. Now ensues a great hiatus, for the organism is unable to appreciate any vibrations between 30,000 per second and 3000 billion per second. Vibrations from 3000 billion to 800,000 billion are appreciated as radiant heat; 400,000 billion to 800,000 billion are appreciated as light. For vibrations from 800,000 billion to 30,000,000,000 billion, and far beyond (3×10^{21}) embracing the ultra-violet rays, the x-rays and the γ-rays, there is no appreciation whatever.[1]

When we consider the vast range and number of the forces at work in the universe, the exceedingly limited capacity of the organism to become cognizant of them becomes very apparent. Living protoplasm fails utterly to develop receptors for these unnumbered manifestations of energy. Protoplasm seems to be "transparent" to them. Have we not a hint here as to the structure of protoplasm? If deluged by them in great volume it may be destroyed, but as ordinarily exposed in the course of nature to electricity, to ultra-violet rays, to x-rays, and other rays, it remains unaffected. It is very suggestive, too, that it is practically transparent to light rays and is obliged to develop a pigment, the stigma, the visual purple, to arrest them. Similarly, it is largely negative to coarse vibrations and requires the development of a vesicle with its contained gross particles of mineral matter.

Save to the very few groups of impacts which have been enumerated, living protoplasm has been almost wholly negative in its reaction to the incident forces of the universe. Extremes of heat and cold, violent physical destruction, have been the most it had ordinarily to contend with. Rarely have other agencies interfered with its existence. Its very "transparency" has been its salvation. Perhaps it is its complexity, its semifluidity, its inability to take up manifold modes of motion, its colloidal plasticity, its peculiar molecular structure, that have made possible the passage through it of such a vast array of forces without change in its substance. In their passage through, they may possibly influ-

[1] Herrick, "Introduction to Neurology," W. B. Saunders Co., Philadelphia and London, 1927, fourth edition, p. 81.

ence its physics or its chemistry, but of this there is no direct evidence. They appear to pass through with as little influence as the waves of the broadcasting radio pass through our own bodies. However, with these questions we are not at present concerned.

It now becomes necessary to approach the subject from another aspect. Our own reaction to the environment, like that of other metazoa, is intimately linked with our nervous apparatus. Let us therefore briefly consider the basic facts presented and the inferences which these facts justify. In order that a comprehensive conception of the reaction of living protoplasm to the environment may be formed, let us begin with its very simplest expression. When the pseudopod of an amoeba comes in contact with a foreign body, the pseudopod flows around the foreign body and thus takes the latter into the interior of its own mass; or, the pseudopod is withdrawn. The first mode of reaction takes place when the foreign body is capable of serving as food, the second mode when the foreign body is incapable of serving as food. Should the foreign body be made up both of material capable of serving as food and of material incapable of serving such a purpose, the two are separated; after a time the first disappears, apparently becomes a part of the substance of the amoeba; the second is ejected. These reactions are of course not to be explained on the basis of a "volitional" behavior on the part of the amœba but upon the purely physical, physico-chemical or electro-magnetic reactions of the constituents of the protoplasm with the materials of the foreign body. We are here reminded of the reactions which are observed in the white corpuscles of the blood which have retained the power of extending and retracting portions of their substance in every way comparable to the pseudopods of the amœba. It would be equally and maifestly absurd to attempt an explanation of these phenomena upon a volitional basis. However, we are not here concerned with their interpretation but merely with the recognition of the fact that contact with foreign bodies results in living protoplasm, under given conditions, in movement. When we turn our

attention to the sponge, one of the simplest metazoa, we find that the epithelial cells in the pore canals behave very much as do the smooth fusiform muscle fibres of other metazoa. The water with its contained substances passing through the canals causes under given conditions a contraction of these cells and thus a narrowing or a closure of the canals. At the same time the opening of a pore may be closed by the dermal membrane which consists of undifferentiated syncytic tissue and which may flow and coalesce over the opening; and at the same time the body of the sponge as a whole may undergo shrinkage or contraction. The last two factors are of very general significance only. It is the behavior of the smooth muscle-like cell which is of immediate interest. This cell evidently responds to direct contact stimulation; there is no trace of a nervous organization. Cells that respond to direct stimulation without nervous intervention are found also in many of the higher metazoa but to these we need not at present give our attention.

In metazoa of a greater complexity than sponges, as in jelly-fishes and sea-anemones, the muscle cells no longer receive their stimulation from immediate contact. An epithelial cell which itself is quiescent receives the impact and transmits the impact to a subjacent cell which contracts. This second cell is the muscle cell. The receiving cell has been termed the "receptor," the muscle cell the "effector." Later a third cell appears between the receiving cell and the muscle cell. The function of this third cell appears to be to diffuse and to distribute to the muscle cell or to a number of muscle cells the impacts derived from the receiving cell. Its function is that of an intermediary. It has been termed by George H. Parker the "protoneurone." The three cells together thus clearly present the prototype of the sensory, nervous and muscular mechanism of the higher animals. A group of receiving cells may be in relation with a group of the intermediate cells their prolongations forming a network with the prolongations of the latter and the impacts received by a few receiving cells may thus be transmitted to many muscle-cells.

In the higher animals the intermediate cells may be increased greatly in number, the new neurones that make their appearance being now termed intercalary neurones. The effect of an intercalary neurone may be twofold; first, it may reenforce, *i.e.*, increase the volume and intensity of the transmission; second, it may come into relation with neurones other than the ones between which it is interposed and thus make possible a more extensive or more complicated response.

The above facts obtain no matter by which receiving cells the impacts find entrance, whether it be by the receptors for contact, *i.e.*, for touch, for smell, for taste, for hearing or for sight. The details of structure of the various sensory organs, of the pathways of transmission and of the central aggregations of neurones into which these pathways lead, do not here concern us. This is equally true of the emissive, the motor and other responses, which ensue upon the entrance of the impacts. The all important fact to be recognized is that the mechanism is purely one for reception, transmission and emission, and that for its function it depends entirely upon the impacts received from the outside world.

The responses in primitive organisms are simple. In higher organisms they become complex; there is an increasing adaptation of the responses to the impacts of the environment. Such an increasing adaptation can only become possible through the multiplication of intercalary neurones. This multiplication permits alike of an increased complexity and an increased adjustment or adaptation of the responses. In ourselves, the responses of the spinal cord and of the brain stem are fixed and invariable. To the brain stem we must add the cerebellum. This leaves as the only structure permitting variable responses the cerebral cortex. The latter, which is also spoken of as the neoencephalon in contra-distinction to the brain stem or paleoencephalon, occupies a position at the end of the primitive neural tube; that is, at the forward end of the developing central nervous system. Unlike the spinal cord and brain stem, it has no relationships to the body segments; therefore, no receptors, no effectors, no direct rela-

tionship to the outside world. It can therefore only be in relation with, and grow in relation with the anterior extremity of the neural tube. Its neurones in their development and multiplication can only establish relationships with and through the brain stem, *i.e.*, through the primitive segmental brain or paleoencephalon. Ingress and egress are possible only through the latter. For us the important fact is that not having segmental relationships,the neurones of the endbrain are necessarily limited to the function of intercalary neurones. Another inference that is unavoidable and conclusive is that if the responses of the cortex to impacts are "variable," "adjustable," "adaptable" and therefore capable of change, the neurones of the cortex cannot bear the same fixed relations to each other as do the neurones of the brain stem and cord. The actual physical relationship of the neurones of the cortex, *i.e.*, of the neoencephalon do not immediately concern us as long as this fact is recognized. Whether we accept the view that the relations of the neurones to each other are modified by an extension or a retraction of their terminal processes—a view which has been advocated by the writer and others, and concerning which Kappers declares that the amœboidism and, in any case, the neurobiotactic phenomena of nerve cells, have not for a long time been mere hypotheses, but are actual facts—or whether we accept the view of Sherrington, that there is a surface (hypothetical) of separation between the neurones and that such a surface may "restrain diffusion, bank up osmotic pressure, restrict the movement of ions, accumulate electric charges, support a double electric layer, alter in shape and surface tension with changes in difference of potential, alter in difference of potential with changes in surface tension or in shape, or intervene as a membrane between dilute solutions of electrolytes of different concentration or colloidal suspensions with different sign of charge," the fact of the variability of the interplay between the neurones must under all circumstances be admitted.

Let us recall briefly that the proximal processes of the

neurone which receive the impact to be transmitted—known as the dendrites—transmit the impact to the body of the nerve cell, while the body of the cell in turn sends the transmitted and perhaps modified impact through the distal processes, known as the axones, and which through their terminal tufts send the impacts into other neurones, or series of neurones; thence into emissive gate-ways or perhaps into neurones in other portions of the cortex. Kappers regards the fact that transmission takes place through the neurone in one direction only as a neurobiotactic phenomenon. He declares that the formation of dendrites and axones is the result of the reaction to stimuli; the axone is a formed product of the stimuli current; it grows with the current, is formed by the current. The dendrite is likewise a formed product of the stimuli current. In the passage of the current from cell to cell, the axone terminals of the first cell are drawn toward the dendrites of the second cell, and the dendrites of the second cell are drawn toward the axone terminals of the first. The mere act of the transmission of an impulse brings about an approach, the whole process being neurobiotactic. Again, the facts of anatomical structure show that there are extensive and numerous pathways—association tracts—which connect different parts of the cortex with each other. Some of these connecting fibers form extensive and long bundles of fasciculi; others are relatively short; others still connect immediately or closely adjoining areas of the cortex. In fact, the arrangement is such that any one part of the cortex is directly or indirectly connected with every other part. Finally, the conclusion is inevitable that the response to the impacts must be automatic. Such response is clearly automatic when but one neurone is interposed between a receptor and an effector, and this factor does not change when the interposed neurone becomes multiple.

The problem of consciousness, which now naturally suggests itself, does not here directly concern us. However, it may be here stated that certain facts must be accepted as elemental and intrinsically insoluble, just as we are compelled

to accept the elemental fact that when matter is resolved into its ultimate constituents, its protons and electrons, nothing remains but an expression of "energy"; whatever that may be. The fact that living matter is under given conditions attended by the phenomenon which we interpret as consciousness, must be equally accepted as final and of which no adequate explanation can be phrased. Consciousness is attended by "sentiency" or "feeling," *i.e.*, by a change which is the direct result of the impacts received from the external world. An attempt to discuss the stage in the evolution of organisms at which "sentiency" makes its appearance is of course nugatory. If we are to assign this quality to the amœba, whose reactions there is every reason to believe are the results of purely physical, chemical or electrophysical causes, we must also assign sentiency to the white blood corpuscles. We cannot stop here, however, but are logically forced to assign the same quality or qualities from which "sentiency" may develop to the organic and other constituents of the protoplasm and indeed to the very protons and electrons into which all matter finally resolves itself. However, vague such inferences may seem, the evidence furnished by the metazoa, especially by the more complex forms, is that "sentiency" in some form or forms sooner or later in the course of development makes its appearance. On what other ground are we to explain the complex sense organs of insects, the eye of the cuttlefish, the arrangements in fishes for the reception of the impacts of contact, of coarse vibrations, of sound vibrations, of chemical impressions, of the vibrations of light? and the transmission of these impacts to central nervous aggregations? The kind of consciousness that constitutes the simplest forms of sentiency must be very different from that manifested by the higher forms. In ourselves and doubtless in our kin, the anthropoids, consciousness is mainly though not wholly a property of the telencephalon, that is, of the cerebral cortex; and it has here attained a highly complex expression. For us, at this stage of our inquiry, the cardinal fact to bear in mind is that "sentiency," the recognition of the "state of being or exist-

ence," no matter how simple or how complex, is the direct outcome of the impacts received from the external world. The fact that we in ourselves divide the various forms of receiving apparatus into those which receive impacts from sources external to the body and which, following Sherrington, we term "exteroceptors," and those which receive impacts arising within the body we term "interoceptors" and "proprioceptors," in no way alters the truth of this statement. In every instance the apparatus conveys impacts received to the central nervous aggregations. It is solely, let us repeat, upon the reception of impacts from the physical world, that "sentiency," "consciousness" makes its appearance. It cannot and need not be further explained any more than the existence or entity of the electron.

From whatever aspect we approach the subject, we are inevitably impressed by the fact of the physical relations of living matter to the external world. That living matter can accept only a few modes of motion and that it is "transparent" to countless other modes of motion, we have already seen. To these purely physical facts, others of a kindred nature may be added. Prominent among these is the fact that time is required for the transmission of impacts. The simpler the response, the shorter is the reaction time; the more complex the response, the longer the reaction time. A simple spinal reflex in the frog varies from one hundredth to two hundredths of a second; the simplest reaction time measured in a human subject in the laboratory ranges from one tenth to two tenths of a second. The increase in the length of time is clearly due to the fact, other things equal, that a larger mass of nervous material must be traversed. The time consumed is evidently lost in some physical process. The transmission of an impact from neurone to neurone is delayed by its passage through the synapses[1]—possibly in the delay caused by the formation of protoplasmic extensions, in the passage of ions from axone tufts to dendrites or perhaps, by the establishment of induction; or by all of these processes combined. Time must further be consumed by the passage of the impact

[1] The approximations between the discharging and receiving tufts.

through the cell processes which are often of great length and by the passage of the impact through the cell body itself. Whatever the process may be only one inference is possible, and that is that the process is physical.

It is, however, in the field of the sensations which result from impacts that the most significant and remarkable facts have been observed. These facts are not new, but they do not appear ever to have received the degree of attention and emphasis which their significance justifies. It is found, for instance, that a sensation aroused by a given impact having been noted, an increase in that sensation can only be brought about by a proportionate increase in the intensity of the impact; that is, the increase in the intensity of the sensation and the increase in the intensity of the impact do not pursue an identical course, for the intensity of the sensation increases in an arithmetic progression while the intensity of the impact must increase in a geometric proportion. For example, a man capable of distinguishing between the weight of 16 ounces and 17 ounces, cannot distinguish between 32 and 33 ounces, but only between 32 and 34 ounces. If a man looks at a light of 10 candle-power, he cannot become conscious of an increase in the intensity of the light until 2 candle-power is added; if he looks at a 60 candle-power flame, 12 candle-power must be added; if it is a 2000 candle-power light, 400 candle-power must be added. We are especially indebted for our knowledge of this subject to a physiologist of a past generation, Weber of Leipzig; more especially in regard to cutaneous and auditory sensations. The facts form the basis of what is to-day known as Weber's law. This may be briefly stated as follows: An increase of sensation depends upon a proportionate increase of the stimulus; the increase of sensation is in arithmetic progression, that of the stimulus in geometric progression; or, to state it in other words, the sensation increases in proportion to the logarithm of the stimulus. In a sense Weber's law is as purely physical as the one which tells us that illumination is inversely as the square of the distance from the light source and must be equally accepted. These facts admit of but one inter-

pretation, and that is, that the reaction of the protoplasm of the neurones to the impacts is determined by purely physical laws and is limited by the structure of the protoplasm.

The above facts lead inevitably and exclusively to the following results; first, that our consciousness is limited to the states aroused by the impacts which the protoplasm is capable of receiving; as pointed out, these are demonstrably few in number. Secondly—and this is even more important—such changes as are induced in the protoplasm of the neurone correspond only imperfectly to changes in the impacts. As we have seen, there are decided gaps between the possible protoplasmic reaction and the series of the impacts. If this be true of so simple a matter as the mere variations in the intensity of the impacts, what shall we say of the variations in the qualities of the impacts? We have no guarantee that corresponding changes take place in our protoplasm. Such a correspondence may, it is true, exist, but the probabilities are strongly against it taking place save in a limited degree. Finally—and this it is hardly necessary at this point to restate—we can only be conscious of the changes taking place in the protoplasm of our own substance. At best these changes correspond very imperfectly to the changes going on in the outside world. Concerning the latter, it follows as a corollary that our knowledge is only inferential.

Under these circumstances what shall we say of the mental pictures that we form, of the relations which these pictures bear to each other, of the sequences which obtain between them, of the thought processes of which they are the embodiment and which form our mental content and, finally, of the reasoning process, the process which constitutes our logical faculty? Finally, what shall we say of the conclusions which we reach?

As to the changes which take place in the protoplasm of the neurone, these again must be such changes—*can only* be such changes—as its structure permits. This structure imposes therefore certain limitations not only as to the sensations which impacts arouse but also as regards the mental pictures

which are formed. The latter depend, of course, upon the inadequate and faulty sensations aroused by the impacts, while their character depends upon the special neurone associations or conbinations to which, again, the impacts give rise. Certainly, no guarantee exists that these pictures represent the actualities of the external world. That, however, faulty as they are, they in a measure or in a degree, follow or correspond to the external changes must, I think, be regarded as incontrovertible. That the content of these pictures, made up in varying degree of light, sound and other reactions, depends purely upon the impacts received from the external world, must be equally admitted. With the course followed in the transmission of a given impact or series of impacts, we have here nothing to do. The course followed depends upon a number of factors. If a given series has been repeatedly received in the past, the transmission results in the formation of the same or similar associations among the neurones. If the series is new, new associations, new pathways are established. One important fact is outstanding and that is that our conscious experiences are limited to the changes, the reactions, in our neural protoplasm—the changes which result directly from the reception and transmission of the impacts; and from nothing else. Finally, it goes without saying that these changes are only such as the structure of the protoplasm permits; in other words that they are limited not only in their character but in their correspondence to the changes going on in the outside world. All of this is in keeping with the limited possible number and character of the impacts.

The interpretation here presented is, of course, far different from that of Bishop Berkley for the existence and the reality of the outside world is here clearly postulated. The interpretation here presented is strictly physical and not metaphysical. It is strictly in accord with the facts as far as they are accessible to us; and the lesson which they teach us is, I believe, not only a wholesome one but one which it is absolutely necessary to keep ever before us in our endeavor to

understand not only the outside world, but ourselves. This endeavor can never in its ultimate essence be realized, but we can at least keep the known and established facts within the range of such understanding as our structure permits, and refrain from pursuing the will-o'-the-wisp of metaphysics or falling into the abyss of mysticism.

The first fact that we must admit is that something exists outside of ourselves. We receive impacts, blows, from this something; in fact, it is the reception of these impacts which arouses in ourselves the sense of existence, consciousness.[1] Consciousness is a quality of living protoplasm which when present must vary in degree from that questionable and dawn-like state present—perhaps—in the protozoa and simplest metazoa, to those states potentially existing in the more complex organisms and appearing in their highest expression in the higher mammalia. These states of consciousness, to repeat, must be made up of—cannot but consist of—the physical changes brought about in the protoplasm by the limited number of the impacts which the protoplasm is capable of receiving; and naught else. The impacts received by the central nervous system—in ourselves the cerebral cortex—include necessarily the impacts received from the various other portions of the body; these constitute the somatic or visceral impacts, received through the interoceptors and proprioceptors so-called. These impacts naturally enter into that summation of physical changes of which the totality of consciousness is made up. That in this totality of consciousness certain of the physical changes may predominate is evidenced by our experience. According to circumstances, one or several of the changes induced by impacts from this or that, or from several sense organs may predominate; or it may be that under given circumstances, the somatic impacts may enter in a varying degree or may even play a role in dominance.

In tracing the transmission of the impacts through the nervous system in relation to consciousness, it is necessary to

[1] To substitute for the word consciousness the word "awareness" does not in the least add to our knowledge.

point out that this quality in mammals is absent in fixed responses; such as take place in the spinal cord and in the brain stem. The function of consciousness, as *we* know it, is limited to the neoencephalon, the cortex. Further, responses which are at first accompanied by consciousness may by frequent repetition lose this quality. Many of the acts acquired in early life—the use of utensils in eating, the adjustments of clothing, the movements of writing, the movements concerned in playing a musical instrument—are all acts usually at first performed slowly and with difficulty, but later with increasing ease until consciousness no longer enters into them. The same movements frequently repeated necessitate the constant repetition of the same associations, the same combinations among the neurones, and sooner or later the movements acquire all the character of *fixed* responses. The inference is justified that consciousness disappears in proportion as fixation is established. Fixation of response means the disappearance of consciousness. This inference leads to other no less interesting, an inference that follows as a corollary, namely, that consciousness is present only in the "adjustable" responses; that is, only in those responses which are attended by an actively changing relationship among the neurones. An impact transmitted from the cord and segmental brain into the neoencephalon brings about definite changes in the synaptic relations of the neurones to which the impact is first transmitted. Thence the impact is transmitted to other intercalary neurones, indeed, to many series of the latter, in the manner already indicated. The transmission of the impact, subject to re-inforcement, may continue until motor centers—*i.e.*, until neurones in relation with motor neurones of the segmental brain or cord—are reached, when an outward or motor expression results; or the transmission may continue to diffuse variously through the cortex without a so-called motor area being involved. Whatever the course of the transmission, it is inevitable that the neurones concerned are involved in sequence. The axone-terminals of the first neurone come into functional, *i.e.* physical relation with the

dendrites of the second. The second neurone effects a like synaptic relation with the third, the third with the fourth, and so on. It follows that just as soon as an impact has been transmitted by a neurone—*i.e.*, just as soon as it has completed its discharge, the synaptic relationship is broken and the neurone is again at rest. It is, I believe, a legitimate inference that a neurone at rest can have no relation with consciousness; a neurone at rest is, so to speak, unconscious. It follows, therefore, that consciousness is only present in the neurones that are actively concerned in transmission. Consciousness is, therefore, itself a phenomenon of cortical transmission. Further, it is the summation of the activities of all the neurones aroused at a given time which constitutes at that time the conscious individuality. Consciousness must therefore be regarded as made up of many integers. In character it must vary from time to time according to the impacts received. The extent of the conscious field must depend upon the number of neurones active at a given time. The common activity, that is, the united group activity of a given number of neurones, gives rise to a "community" of consciousness, a sense of "self"; something distinct from the outside world.

Again, a transmission having found its way through the cord and brain stem to the cortex, the associations or combinations successively formed among the neurones are the same or similar to associations formed upon the previous receptions of similar impacts. If the impacts vary, the associations may assume relations in part to former neurone combinations or may result in combinations partly new. If the series of impacts be wholly new, the resulting associations are, other things equal, wholly new. In any event, a transmission having entered the cortex, many thousands of neurones are engaged in the activity. The pathway pursued may depend upon the previous establishment of pathways of least resistance; *i.e.*, as to whether previous similar impacts have been received and transmission thus "facilitated"; or the transmission may pursue pathways entirely new, but by no possibility

can anything new in the physical sense be added. Nothing new can be created. New combinations, new associations among the neurones is all that can take place. New relations of incoming impacts, new relations among neurone associations, radical changes in the associations among each other may supervene, but that is all. No change in the intrinsic character of the transmission is possible. It is hardly necessary now to state that it is the transmission through the cortex which constitutes "thought." This is true no matter what the course of the transmission. It may or may not diffuse extensively through the cortex; it may or may not eventuate in a discharge through an emissive gateway. In any event, we are forced to the conclusion that the physical, the biological, interpretation of thought is absolutely essential to a scientific conception of its nature, and, also, of its limitation.

Thus far the following facts and deductions have presented themselves. First, the existence of something outside of ourselves; an external world. Second, the limitation of our sensations to the kinds of impacts which we are capable of receiving. Third, the fact that consciousness is limited to reactions which are constantly changing adjustments or adaptations to the inpouring impacts. Fourth, when the necessity for constant readjustments or readaptations has disappeared and fixation has taken place, the reactions are no longer attended by consciousness. Fifth, the possibilities of thought are limited by the structure of the protoplasm, as evidenced not only by its inability to receive any but a very limited number of the vast array of forces existing in the outside world, but also by its inability to react save in a limited manner to those which it does receive.

Further, our visual conceptions, our mathematical interpretations, the factors into which we attempt to resolve the universe, are all of them limited by the structure of the protoplasm of our cortical neurones. For the solution of certain abstract mathematical problems, *i.e.*, problems never presented by the outside world, our protoplasm is clearly inadequate. One or two time worn illustrations will suffice; for

example, the construction of a cubical container that will hold just one cubic foot of water is a simple matter, but no one has yet been able to calculate the exact size of a cubical container that will hold just two cubic feet of water. The same is true of the squaring of the circle. The structure of the neural protoplasm is such that it cannot take on the motions that conceivably would be required. As just stated, in the outside world such problems have no existence; they are simply abstractions that correspond to no reality. When the problems deal with actualities, great results are achieved as witness the accuracy of the calculations of the astronomer and in modern times the remarkable achievements of the physicist. Regarding the final results of mathematicians, we should, however, be able to visualize them. If they are such that they cannot be translated into visual pictures, the inference is justified that they are open to suspicion and distrust. In the field of abstract conceptions we clearly tread upon dubious ground. Abstract conceptions represent nothing that actually exists in the outside world.[1] At most they are artificial pegs upon which to hang our logic.

It is with much hesitation that the writer now enters upon a discussion of the modern interpretation of the external world, and yet if the inference which the facts and deductions as to the impacts received from the external world and as to the reactions of the substance which receives these impacts are to be accepted, we have no choice but to pursue our inquiry to a conclusion. In the beginning of this essay, we called attention to the modern conception of the structure of the atom according to which the atom consists of a nucleus about which a given number of electrons are revolving; and it was further pointed out that to this interpretation there have been added two other attributes, namely, the "quantum" and the "wave." Do the considerations so far advanced in this essay prove of any help, do they aid us in any way, in the at-

[1] In the mathematics of abstract problems, the interplay of the various factors is merely an interplay in our neural protoplasm. There is no guarantee that it represents any reality in the outside world. The only test of a final result is its objective application. If such a test be not practicable, it is assuredly not asking too much that the result should at least be of such a character that it can be visualized.

tempt to form a mental picture of the atom with these added qualities? Let us take up the "quantum" first. We have already defined the quantum as the energy which is given out or received at intervals by an atom; that is, the energy (light) is given off discontinuously. On the other hand, the absorption of energy (light) is also discontinuous. Finally the quantum of a given wave-length is the same no matter with which of the 92 different kinds of atoms it is associated. According to the conceptions originally developed by Planck in 1900, the quanta of energy are related to the frequencies of oscillations of the electrons. Later Planck embraced all forms of energy in his interpretation; in fact the latter applies to all forms of atomic behavior.

The quantum, according to Bohr, is the result of the "jump" of an electron, as in the instance of hydrogen, from an outer ellipse to an inner ellipse. Such a jump is really a fall of the electron toward the nucleus. The jump results in a given loss of energy and it is this energy which is "radiated." During the ensuing stationary stage, energy is again absorbed by the electrons from the ether waves. With the causes of the jump from one ellipse to another, or the causes of the reverse process in the period of absorption, or with other details, we are not here concerned.

Before attempting to form our mental picture, we are further constrained to consider the view that electrons are not small particles but are "waves." This view has been very clearly presented by C. J. Davisson.[1] In endeavoring to account for the regular reflection of a stream of electrons from the surface of a nickel crystal he encountered a difficulty which he expresses in the following analogy: "If we were to fire a load of bird shot against a pyramid of cannon balls, we should not expect to find a little cloud of shot moving off in the direction of the regular reflection from the face of the pyramid. A surface made up of cannon balls is much too coarse grained to serve as a regular reflector for particles as small as bird shot.

"The analogy is not such a good one really, for we do not

[1] *Journal Franklin Institute*, May, 1928, p. 597.

think of electrons rebounding from the surface of an atom in the way that shot rebounds from a cannon ball. We have been accustomed to think of the atom as rather like the solar system—a massive nuclear sun surrounded by planetary electrons moving in closed orbits. On this view the electron which strikes into a metal surface is like a comet plunging into a region rather densely packed with solar systems.

"There is a certain small probability, or at least there might seem to be, that the electron will strike into an atom in or near the surface of the metal, be swung about comet-wise, and sent flying out of the metal without loss of energy. The direction taken by such an electron as it leaves the metal should be a matter of private treaty between the electron and the individual atom. One does not see how the neighboring atoms could have any voice in the matter. And yet we find that the high-speed scattered electrons have a preference for moving off in the direction of regular reflection, a direction which is related to the plane of the surface." Dr. Davisson points out that if electrons were waves there would be no difficulty. Here again, we are not concerned with the detailed discussion but only with the influence which the wave conception of the electron imposes upon the mental picture we are trying to form.

And now two other interpretations must be added. One is that of the conception of the "dynamics of matrices." This has especially to do with the difference between the values of energy before and after the transition, that is, the jump of the electron from one ellipse to another. Various mathematicians, Heisenberg, Bern, Jordan, Dirac[1] and others have attempted to calculate the emitted frequencies from the charge of the electron, its mass and Plank's constant, and have established values which lead to Balmer's lines. The conception of the "dynamics of matrices," in itself so interesting and important, adds, as we will see, no difficulty to the formation of a mental picture.

Finally we have still to consider briefly the "atmosphere"

[1] See Lorentz, *Journal Franklin Institute*, April, 1928, p. 458.

of Schrödinger. The electron of the hydrogen atom is now "replaced by something that is distributed all over the space surrounding the nucleus." [1] Its density rapidly diminishes as the distance from the nucleus increases. Schrödinger has been able to assign to any point of the atmosphere a certain quantity which may be considered as the density of an electric charge. "Its values are such that the total charge is equal to that which formerly was attributed to the electron, so that now, at distant points, the action of the nucleus is neutralized by that of the atmosphere. Finally, the wave equation shows that there can be cases in which the distribution of electric charge is not invariable but fluctuates periodically with a frequency just equal to that of the radiations emitted by the atom." [2] Lorentz closes his interesting paper "How Atoms Radiate," [3] with the assumption "that according to circumstances, the charge may be either concentrated in the shape of an electron, or much more widely diffused."

What now are the mental pictures we are justified in forming? or, rather, what are the mental pictures that it is *possible* for us to form? what are the mental pictures that the structure of the protoplasm of our cortical neurones permits? What is demanded of us by the interpretations of the physicists here presented?

As stated in the beginning of this essay, the interpretation of the structure of the atom as consisting of a nucleus and revolving electrons was so simple and so clear that an adequate mental picture could readily be formed; and yet if we attempt to translate this picture into a concrete expression of a reality, a difficulty at once arises. What of the space between the nucleus and an electron? This is supposed to be relatively very great. It is clearly no exaggeration to say as does Professor Bazzoni that a proton of hydrogen having the size relatively of a pea and situated in New York City would have its electron of a much larger size so far away as Buffalo, N. Y. [4]

[1] *Loc. cit.*, p. 470.
[2] Lorentz, *loc. cit.*, p. 470.
[3] Lorentz, *loc. cit.*, p. 471.
[4] "Kernels of the Universe," by C. B. Bazzoni, 1927, pp. 77, 78.

The question arises, is there nothing but "nothingness" between these two entities, the proton and the electron? We are so constituted that we cannot form a picture of "nothingness." Again, when we are told that the electron of hydrogen may jump from one ellipse of rotation to another, the mind naturally asks "across what is the jump made"? Is it across "nothingness"? Again, what shall we do with the "atmosphere" of Schrödinger? Are we to accept the final assumption of Lorentz, evidently intended as a solution of the problem, that the "charge may either be concentrated in the shape of an electron, or much more widely diffused." Does this assumption help the formation of our mental picture? I think not, it really adds to our difficulties.

Further, what shall we say of the quantum which according to Bohr is the result of the "jump" of an electron from one ellipse to another. The jump results, as just stated, in a given loss of energy which is, as a consequence, "radiated." How shall we reconcile with the above interpretations the view that the electrons are in reality "waves"? How shall we incorporate them in the mental picture we are endeavoring to form? Finally what shall we do with the matrix?

It seems to the writer that we are forced to follow the necessities imposed upon us by the structure of our neural protoplasm. The inevitable result is that in addition to the picture of the proton and the electron, we must picture to ourselves "something" between them. Indeed, this leads to another possible detail. These entities, the proton and electron, may be merely fast revolving, spinning, foci of energy in a tenuous medium—a medium that fills all space, that is space. In his interesting and lucid presentation of the discoveries made in physics since the end of the 19th Century, in volume III, page 144, of the Encyclopaedia Britannica, 13th Edition, Millikan in speaking of ether waves, expresses himself as follows: "This continuous passage of frequencies from a thousand billion billion per second, over into a zero frequency, *i.e.*, over into static electrical fields, all these waves possessing identical characteristics as to speed of propagation, as to polarization

and as to relations of electric and magnetic vectors, demands one and the same transmitting mechanism or medium to take care of them all, whether a "world ether" or "space," this last term meaning no longer emptiness but emptiness endowed with specific properties, if such a Hibernicism suits one's taste." Clearly the existence of a medium, call it ether or what one will, is a necessary condition of human thought. A tenuous medium lends itself readily to the picturing of waves. What seems more natural? Waves in water, waves in the media about us, waves in the medium of space?

Again, the sizes which physicists have assigned to the entities, the electron and the proton, are so excessively minute that they are inconceivable and—dare I say it?—apochryphal. Further, have we not the right to apply to them, as to all other masses, the principle of relativity according to which mass and energy are interchangeable? If this be true, the size neither of the proton nor the electron can be regarded as fixed. Does not this pave the way for the transition of the electron into the atmosphere of Schrödinger? Whether we express the energy in terms merely of "energy" or in terms of "electricity" in no way affects our picture.

Finally, what whall we say of the "matrix"? Does not this simply express the multiple pathways, the ellipses, in both the stationary and the emissive periods of the atom, in which the electrons move? Certainly this is not irreconcilable with the picture we have thus far formed. Indeed, the latter lends itself to some elaboration. Do not Balmer's lines, corresponding to the ellipses of the electron of hydrogen, suggest in their relation to each other a "nodal" series? Have we not, in other words, an analogy here to Lagrange's weighted string? [1] Is it not true, perhaps, after all, that the essentials of the behavior of the universe are intrinsically the same throughout? Is it not true finally that the only mental pictures we can form are those embodying mass and energy, the mass being pictured for the moment as "something station-

[1] The analogy of the "stretched string" evidently also occured to Lorentz. *Loc. cit.*, p. 460.

ary" and the energy as "something moving"? Indeed, the protoplasm of which our conscious selves is made permits of nothing else. We may allow the results in our calculations to become extremely complicated, but unless the pictures can be reduced to these simple elements, the results are nugatory. Finally to the question, can we ever know the outside world as it really exists, we must return a negative answer.

THE ORIGINS OF CIVILIZATION IN AFRICA AND MESOPOTAMIA, THEIR RELATIVE ANTIQUITY AND INTERPLAY

By GEORGE A. BARTON

(*Read April 20, 1929*)

WHETHER North Africa or Mesopotamia and contiguous lands was the home of the earliest civilization, depends upon what we call civilization. Do we mean the earliest continuous evolution of peoples which we can trace in these lands, or do we mean the beginnings of the civilizations which continued into the period now covered by written history?

The long and fascinating story as it can now be discerned begins in North Africa after the last glacial period, when the Sahara and contiguous regions were fertile. There then lived in this region—the region south of the Mediterranean Sea including the Sahara—a stock which we call Hamitic—a not altogether happy name derived from the 10th chapter of Genesis. The purest examples of the early Hamitic speech still survive among Hamitic tribes north of the Sahara—the Tamesheq, the Kabylee, the Shilch, and the Riff languages. Ancient Egyptian, from which Coptic is descended, is a Hamitic language profoundly modified by mixture with Semitic speech. The Bedauye, Belin, Saho, Irob-Saho, Afar, Somali, Galla, Chamir, and Kafa languages of Abyssinia and Somaliland are Hamitic languages modified by Kushite admixture. The Nandi, Norobo, Masai, and Mblunge, of the African lake-region, are Hamites mingled with a negroid stock. The Ful and Zenanga, on the southern border of the Sahara in central West-Africa, are Hamites mingled with Negroids who spoke languages of the Sudanese type. Finally the Nama, in western South-Africa, are Hamites mingled with Bushmen. These Hamites possess, in spite of mixtures, certain characteristics in common, which ethnologists recognize, and their lan-

guages, in spite of profound modifications from mixture, possess in common strongly marked characteristics. For example, the pronouns—the most *sui generis* part of speech—are practically identical in them all. They all form intensives by doubling a stem or a root; causatives, by the use of the letter *s*; reflexives, by using the letter *t*; passive-reflexives, by the letter *n*, which, in some dialects, is philologically changed to *r*, and in others to *m*. They differ from all other African languages in dividing all objects into two classes: animate objects, large things, subjects, and males are all of one class, generally called the masculine gender; inanimate things, small things, objects, and females are put in the other class, commonly called the feminine gender. The sign of the masculine is *w* (*u*); of the feminine, *t*. They also possess in common a tendency which has been called "polarity"—the tendency to arrange things in masculine and feminine pairs. They also possess in common the vowel-gradation known as "Ablaut." These and other features which need not be mentioned here differentiate the Hamitic languages from all other African tongues and clearly point to an original racial unity. So far as I can ascertain from the works of ethnologists accessible to me, all the peoples of North Africa were dolichocephalic. They also belonged to the white race. The peoples who speak, or who spoke, Semitic languages, were the Arabs, Akkadians (Semitic Babylonians), Assyrians, Amorites, Canaannites, Phœnicians, Carthaginians, Aramæans, Hebrews, the Abyssinians and other descendants of the ancient peoples of South Arabia. In spite of exceedingly large admixture in some cases with other races, the languages of the Semitic peoples are as closely akin as are the modern languages descended from Latin—Italian, French, Spanish, Portugese.

There is incontestible evidence that the Hamitic and Semitic languages were originally related to one another. They are alike in the following points: (1) They possess a peculiar "pressure-articulation," which arises either from a peculiar form of throat, or from a long racial habit of using the throat. This led to the existence in both groups of a number of pecu-

liar articulations not found elsewhere. The best known of these are the letter Ayin—a sound exceedingly difficult for other peoples to make—and the letter Ḥeth. The Semitic tongues possess also a secondary phenomenon derived from this "pressure-articulation," the frication of certain letters—*b*, *g*, *d*, *k*, *p*, and *t*.[1] (2) In both groups of languages the roots consisted solely of consonants—never of a vowel—and many of them were bi-literal; never were they more than tri-literal. This is a striking peculiarity, and points clearly to community of origin. (3) The pronouns of all the Hamitic and Semitic languages are practically identical. (4) Semitic languages, like the Hamitic, manifest the tendency to polarity already referred to. (5) In both groups derived stems, expressing modifications of the root meanings, are formed in the same way—intensives, by doubling the root or its middle letter; causatives, by the letter *s*; reflexives, by the letter *t*; passive-reflexives, by the letter *n*; and causative-reflexives, by the letters *st*. The linguistic evidence clearly establishes kinship. This is confirmed by skull-measurements. Where the original Semitic type is purest, as in Arabia, the majority of skulls, so far as published measurements enable us to form an opinion, are dolichocephalic. Among the Hebrews, where there was in ancient times a large admixture of the Hurri-Hittite Asiatic stock, the original dolichocephalic type has been superseded by the brachiocephalic type.[2]

As many of the linguistic phenomena which Hamites and Semites possess in common appear in the Hamitic languages in a more primitive form than in the Semitic, the one theory which satisfies the facts is that the Hamito-Semitic race originated in North Africa and the Sahara region, and that at a very early time—say 10,000 to 8000 B.C. or earlier—some of this stock migrated to Arabia—probably South Arabia via the Straits of Bab-el-Mandeb—where they spread over the peninsula in the course of subsequent millennia. As Arabia suffered desiccation, in common with North

[1] For a fuller description of pressure articulation see W. H. Worrell, *A Study of Races in the Ancient Near East*, New York, 1927, p. 57 f.

[2] For a more complete treatment see *American Historical Review*, XXXIII, pp. 763-767 (July, 1928).

Africa, they were gradually forced to migrate in various directions in search of subsistence. It was under this pressure that, by migration and mingling with other races, the various Semitic nations of history, other than the Arabs, were formed. Possibly the Arabs are Hamites mingled with a still earlier population of Arabia.

We are now in a position to discuss more intelligently racial elements in the origins of civilization. If we could call these tribes civilized at the period of the world's history before Hamites and Semites separated, we should be forced to confess that their civilization originated in North Africa. Into the civilizations of Babylonia and Egypt as known to history other elements, however, entered. Let me sketch first the rise of Babylonian civilization.

In the making of Babylonian civilization three distinct races had a part. Two of these, the Semitic and the Sumerian, have been long known; the third has been brought to light only since the Great War. It is known to us through a peculiar type of decorated pottery found at Al-Obeid, Ur, Abu-Sherein (Eridu) and Kish. This pottery is of the same type as that found in the earliest strata at Susa, as that found by Speiser on the east of the Tigris in ancient Assyria, by others at Nineveh, Carchemish, also at Erivan (always in the lowest strata), and by Pumpelly at Anau east of the Caspian. From a skull found at Kish, we infer that this people was brachiocephalic.[1] The members of it who settled in Elam were sufficiently civilized to develop a pictographic system of writing.[2] It is the writer's present opinion that the branch of this race which settled in Babylonia also employed pictographic writing, and that the tablets found at Jemdet Nesr[3] and the pictographic tablet found at Kish[4] will turn out to be examples of the writing of this people rather than of the

[1] See the article of L. H. D. Buxton in Langdon's *Kish*, Paris, 1924, pp. 115–125. Langdon wrongly, as I believe, takes the skull to be Sumerian. The evidence of the skulls found at Al-Obeid, mentioned below, are, however, against this view.

[2] See Scheil in de Morgan's *Délégation en Perse*, VI, pls. 12–23.

[3] Published by Langdon in *Oxford Edition of Cuneiform Texts*, Vol. VII. Langdon believes the writing to be Sumerian, but it is difficult to establish the racial origin of pictographic writing until it has been read.

[4] Published in Langdon's *Kish*, pl. XXXI.

Sumerians. This race appears to have come from Central Asia and to have approached Elam and Babylonia along the east bank of the Tigris.

On the west the Semites were pushing up into Babylonia as fast as the formation of irrigated swamps permitted habitation. They were much less highly civilized than the Asiatic race and at this time contributed to civilization mainly their racial characteristics.

Certainly as early as 3500 B.C. and perhaps as early as 4000 B.C.,[1] the Sumerians, pushing up the Persian Gulf from the south, made their way into Southern Babylonia, driving themselves in like a wedge between the other two races. The memory of their coming is reflected in the myth of Oannes, recounted to us by Berossos, who taught men how to construct houses, till the earth, compile laws, and all other useful knowledge. It is clear that the Sumerians were highly civilized when they entered Babylonia; they knew the arts of agriculture by 3500 B.C.; they could make beautiful objects of gold and silver, surpassing in craftsmanship and beauty anything found in Egypt until centuries later; they could write; they had invented the principle of the real arch and dome; and they had invented the use of the wheel and had chariots.[2] Their writing and pottery[3] show that they did not come from Elam, but it is clear that they brought their civilization with them, having previously developed it elsewhere. Two attempts have been made in recent years to connect their cradleland with India,[4] but the evidence will not bear that inference.

[1] Most recent writers on Babylonian chronology have erred in making the time too short. This is true of Kugler's later work, of Weidner, Ed. Meyer, and Sidney Smith. The fall of the third dynasty of Ur is now fixed astronomically by an eclipse of the moon as having occurred in 2282 B.C. (see Langdon and Fotheringham, *The Venus Tablets of Ammizaduga*, Oxford, 1928, p. 82). Some of the earlier dynasties doubtless overlapped, but, I am convinced, not as much as has sometimes been supposed. The Sumerians were making articles that surpassed the craftsmanship of Egypt before the establishment of the first Egyptian dynasty.

[2] For examples of Sumerian work see Leon Legrain in *The Museum Journal*, March, 1928, pp. 1–34, and C. L. Woolley, *The Sumerians*, Oxford, 1929, chapters I and II.

[3] They employed the potter's wheel centuries before it was employed in Egypt; see Woolley, *The Sumerians*, p. 187.

[4] Cf. M. Rostovtzeff, "The Sumerian Treasure of Astrabad," *Journal of Egyptian Archæology*, VI (1920), pp. 4–27, and L. A. Waddell, *The Indo-Sumerian Seals Deciphered*, London, 1925.

The writing found at Harappa and Mohenjo-daro is of absolutely a different origin from the Sumerian.[1] Further, there is evidence from both the Sumerian script and religion that they came from a mountainous homeland, and the Indus-Valley is not such. Sir Arthur Keith's study of the Sumerian skulls found at Al-Obeid showed them to be of the same general type as the Arabs.[2] I suspect that they may have been descendants of the pre-Semitic population of Arabia and their civilization may have been developed in Oman in Southeastern Arabia, whence they were driven by the gradual desiccation of the country,[3] but at present this is no more than a theory. About 3000 B.C. the clearer light of history breaks upon Babylonia, and soon after 2800 B.C. Semites began to contest the supremacy of the Sumerians.

If, now, we turn to Egypt, Messrs. K. S. Sandford and W. J. Arkell have, working under Professor Breasted's direction, recently made clear to us how many centuries man has been resident in the Nile Valley.[4] For a clear and authoritative view of the development of civilization in Egypt one turns to W. M. Flinders Petrie's *Prehistoric Egypt*, London, 1920. Petrie stands foremost among those who have made us realize that a potsherd, or a statuette, or a comb, or a bead may be as much of a historic document as an inscription.

Petrie divides the time between the beginning of the accumulation of Nile mud up to the founding of the first dynasty into 80 strata or sequence dates. Thirty of these had passed before he finds any traces of neolithic man. This leaves fifty sequence dates or periods distinguishable from the pottery and other archæological objects found in tombs. These Petrie numbers consecutively from 30 to 80. During the millennia covered by this series, he is able from the archæ-

[1] See the writer's "On the So-called Sumero-Indian Seals," *Annual of the American Schools of Oriental Research*, Vol. VIII, 1928, pp. 79–95.

[2] In H. R. Hall and C. L. Woolley's *Ur Excavations*, Vol. I, *Al-Ubaid*, London and Philadelphia, 1927, pp. 214–240.

[3] See the writer's article, "Whence Came the Sumerians?" soon to appear in the *Journal of the American Oriental Society*.

[4] *First Report of the Prehistoric Survey Expedition, Oriental Institute Communications*, No. 3, Chicago, 1928.

ological objects to distinguish three civilizations, or three periods of civilization. He finds that the people who began the Egyptian civilization at S.D. 30 buried their dead in shallow holes, "with a single black-topped cup, a goat skin over the body, and rarely a rhombic slate pallette. Yet even then they fastened the skin with a copper pin." Immediately after these first immigrants there came a more highly civilized people who made pottery so exactly like that still made in the highlands of Algiers that Petrie feels justified in calling this an immigration of "Lybians." The linguistic evidence already referred to would lead us to call them Hamites. This people was fond of making pottery, but their pots imitated baskets. It is possible to describe with some fullness the civilization of this period, if time permitted. It was almost altogether a stone-age civilization. Copper was not common, but was sometimes used for pins to fasten skins, for harpoons, and for small chisels used in carving.

Beginning at S.D. 38 a people came into Egypt, apparently from the east, whose pots imitated stone-ware. They had retreating foreheads, long pointed noses, and small projecting beards. They wore more clothing and had come from higher, colder regions. Petrie thinks they came from the western coast of the Red Sea, or the peninsula of Sinai, or the Hejaz in Arabia. The maximum of this immigration was at S.D. 38, but it continued until S.D. 41–43. The utensils of this civilization gradually drove out the utensils and wares of the first one. It is possible that these immigrants were Semitic. Traces of the third civilization begin at S.D. 46–50, become more pronounced at S.D. 57, and reach their culmination at S.D. 63. This civilization introduced a pottery much like that found at Susa, seal cylinders of Babylonian type, and so many objects which resemble things found in Elam that Petrie is quite positive of Elamite influence. We should call it the influence of the Central Asiatic civilization. The mediators of this so-called Elamite influence could be no others than the Semites of Arabia. South Arabia from this time onwards became a highway between Egypt and Mesopo-

tamia. It seems probable that with the coming to Egypt of the makers of the second and third of these prehistoric civilizations, many of those Semitic influences, which so profoundly modified the Egyptian language and made it approach the Semitic type of speech more closely than the other Hamitic languages do, entered Egypt. Probably it was at one of these periods that the worship of Osiris and Isis, whose characters so closely resemble those of Tammuz and Ishtar, and whose names are philologically derivable from the Semitic root AŠR, from which the names of several Semitic deities are also derived, entered Egypt. The Hebrew *ashera* and the Assyrian *eshirtu*, meaning "shrine," are derived from the same root.[1]

This wave of Elamite influence was superseded at about S.D. 63 by another which came from Nubia. The stream of Semites had, however, become the intermediaries who carried a knowledge of arts developed in Central Asia to the less civilized Egyptians, and contributed to the civilization which was later to emerge in the Egyptian dynastic period.

Shortly before the rise of the first dynasty another wave of immigration, probably of Semites, made its way into Upper Egypt, perhaps through the Wady Hamamat. Egyptologists seem agreed that it was due to the influx of this new life that another step forward was taken in the evolution of Egyptian civilization. The Semites seem never to have been makers of civilization, but rather transmitters, and, if these Semites from Arabia had anything to contribute to civilization at this time, they could only have learned it from the Sumerians, who had by this time supplanted the Central Asiatic race in Babylonia. Other possible examples of Babylonian influence during the archaic period of Egypt's history have been urged by several scholars: the scene on the ivory knife-handle from Gebel el Arak, now in the Louvre, the decorated door-socket from Hierakonpolis, mace heads, the sis-

[1] The writer is now convinced that the view of the origin of Osiris which he once advocated in a paper read before the Philosophical Society, entitled "Tammuz and Osiris," and which was afterward published in the *Journal of the American Oriental Society* (Vol. 35, 1915, pp. 213–223), is untenable, and that the one stated above is demonstrable.

trum, recessed brick-building,[1] and the step-pyramid, which resembles the form of the Babyloniam ziggurat.[2] Those who have, on the evidence of Babylonian influence made probable by these, contended, like Hommel, that Egyptian civilization was derived from Babylonia, have overworked the evidence, but that Egypt received important influences from Babylonia—influences vital to her progress—appears to me to be made clear by the evidence.

Was Egypt at this period influencing Babylonia? Such influences there may have been, but, if there were, the writer does not happen to know of them. No evidence of such influence is known to him earlier than the Assyrian period. Reinisch finds traces of Hamitic influence in the name of Sin-iddinam, a king of the dynasty of Larsa, about 2120 B.C., but, if such influence is really there, it came from emigrants from Somaliland and not from Egypt. Hamites from East Africa appear to be resident now in Oman, and probably waves of them have from time immemorial surged into Arabia and been absorbed there.

If by civilization, then, we mean the mastery of the arts of life, it must be said that, while the share of Egypt is by no means to be minimized, she was anticipated in time and in many details by the Sumerians and their predecessors, the makers of the painted pottery in Babylonia and Susa, both of which peoples developed their civilizations outside of Babylonia, and from both of which Egypt borrowed.

Indeed, no Hamitic race except the Egyptians has ever developed a high civilization. We may go further and say that no member of either the Hamitic or Semitic race has ever developed a high civilization except under the stimulus of more highly civilized peoples, unless the Egyptians be an ex-

[1] See H. Frankfort, *Studies in Early Pottery in the Near East*, I, London, 1924, 117 ff., and, for the sistrum, Woolley, *The Sumerians*, p. 186.

[2] If it be said that the step-pyramid is older than any surviving ziggurat in Babylonia, it should be answered that Ur-Nina, a king of Lagash, records having built a ziggurat, and he lived before Zoser, the builder of the step-pyramid; see the writer's *Royal Inscriptions of Sumer and Akkad*, New Haven, 1929, p. 21, and his article, "The Form and Nature of the E-pa at Lagash," *Journal of the American Oriental Society*, Vol. 43 (1923), pp. 92–95.

ception; but it has been shown above that they are not an exception. Even had they developed without outside suggestion and stimulus the great pyramids—structures that call forth our highest admiration on account of their mass and the skill of their construction—we should be obliged to admit that the people who invented wheeled vehicles contributed more to civilization, since more of us use wheels than use pyramids!

If we turn from the mastery of material things to social organization, we again find the peoples of Babylonia fully abreast of the Egyptians, and also a little in advance of them in time. If during Egypt's Middle Kingdom we find a social conscience revealed in the "Tale of the Eloquent Peasant," "The Admonitions of Ipuwer," and other such literature, in Babylonia we have the Code of Hammurabi and other earlier Sumerian codes, which reveal the social conscience, not only expressed in literary ideals, but made legally binding in the business of daily life.

A COMPARISON OF EGYPTIAN AND BABYLONIAN CIVILIZATIONS AND THEIR INFLUENCE ON PALESTINE

By ALAN ROWE

(*Read April 20, 1929*)

As you will have seen by the Programme the title of my lecture is A Comparison of Egyptian and Babylonian Civilizations and their Influence on Palestine. Now I wish at the outset to explain that this title needs some qualification as I intend to deal only with these influences as I found them at Beisan, the Beth-shan of the Old Testament, which site is undoubtedly a typical one of its kind for northeastern Palestine; in other words, what influences reached Beisan must also have reached Megiddo, Taanach, Rehob and other great neighboring sites. Also, in order to make the record as complete as possible, I shall duly record all other foreign influences met with at Beisan. After you have heard my paper you will realize, I think, that the greatest influence on the site was Egyptian, the second place being almost evenly divided between the Mediterranean and the Syro-Hittite, etc., while the Mesopotamian influence was present in an almost negligible quantity; in this connection I refer mainly to the period between the reign of Thothmes III, who came to the throne of Egypt in 1501 B.C., and the commencement of the Iron Age about 1080 B.C., which was ushered in with the advent of the Philistines from the Ægean-Anatolian regions. My notes on all these influences will now be presented in chronological form

Of the Palæolithic and Neolithic Ages at Beisan nothing yet can be said as no flint implements dating from those times have so far been discovered in the district. It was in the base of a small narrow trial trench made in the north side of the Beisan tell, and in the great cemetery to the north of the tell, in both instances in graves of about the beginning of the Mid-

dle Bronze Age, 2000–1600 B.C., that were found the earliest skeletal traces of man in Beisan. Each grave in the cemetery consists of a roughly circular subterranean chamber with a small passage leading to it, the entrance of which is blocked by a single large stone. In a good many instances, owing to the poor nature of the rock, the roof has collapsed, thus destroying the body and some of the pottery where these had not already been destroyed by the ancient tomb robbers. The bodies appear to have been laid in a crouching position without any systematic orientation of the corpse. In one of the graves were a large ledge-handled pot, a single-handled jug, an open Canaanite pottery lamp with four spouts, etc. From certain of these interments, including that just mentioned, have been recovered long pointed javelin-heads of bronze. The base of the javelin is twisted into a kind of hook so that the weapon could easily be affixed to its shaft. No articles of jewellery or adornment whatever came from these graves. The Canaanites of this period must have been in a low stage of culture and it is interesting to observe that no objects which could be ascribed to a foreign source were found with the bodies.

The next objects in order of date found at Beisan belong to the Hyksos era and comprise an inscribed jar handle and many scarabs. These were found in various graves in the great northern cemetery and in various levels on the tell, all dating to periods later than the time of the Hyksos. The Hyksos were Asiatics, doubtless Semites, who about 1675 B.C. entered Egypt, the northern part of which together probably with Palestine and Syria they apparently controlled from the city of Avaris in the Nile Delta. They were driven out of Egypt by Ahmose I (1580–1557 B.C.) and the remnants of their power were finally destroyed by Thothmes III (1501–1447 B.C.) when, about 1479 B.C., he crushed the confederacy of Canaanite and Syrian tribes at Megiddo, an important ancient town not very far northwest of Beisan, the site of which is now being excavated by the Oriental Institute of the University of Chicago, under Dr. Breasted's control. Although

nothing is known of local conditions at Beisan at this age it may be mentioned that the Hebrews themselves probably entered Egypt during the Hyksos period and left the country at the expulsion of their patrons or shortly afterwards. An interesting tradition preserved in *The Book of Jubilees*, xxix, 14–16, says that Joseph, the son of Jacob, pastured his sheep near Beisan.

On the Beisan tell nine main city-levels have so far been found, the earliest level dating to the time of Thothmes III. After this king destroyed the confederacy of tribes near Megiddo he seems to have sent an army to take Beisan. During his reign the great temple dedicated to "Mekal the lord of Beisan" was erected. This local Canaanite god is here met with for the first time in the history of archæology and seems to have been a kind of storm and pestilence god, allied to Resheph and Sutekh. In the temple was found in 1928 a magnificent panel bearing the figures of two lions and two dogs, the lion representing Nergal, the god of pestilence and death, and the dog the mythological temple guardian who protected the temple against Nergal. This panel seems to show affinities with the culture of Mitanni, a country to the northeast of Syria, while certain cylinder seals give evidence of Syro-Hittite influence at Beisan in the time of Thothmes III. By far the greatest influence at Beisan during this reign was the Egyptian, which prevailed there (with the exception of during the period from the end of the el-Amarna era to the commencement of the reign of Seti I) until the death of Rameses III in 1167 B.C. Mesopotamian influence was almost negligible while that from the Mediterranean regions, as shown by the pottery and other remains, begins to appear in a somewhat large way.

Thothmes III was succeeded by Amenophis II and Thothmes IV, whose reigns represent the eighth city-level on the tell (1447–1412 B.C.). Judging from the antiquities found in this level, the Egyptian and Mediterranean influences were still strong at Beisan, while the Syro-Hittite was gradually increasing. That from Mesopotamia still seems to have been rather small.

The next king of Egypt, Amenophis III (1411–1375 B.C.), was the founder of the seventh city-level. The plan of the temple erected on the tell during his reign is very similar to the plans of certain of the tomb-chapels, shrines in private homes, and a shrine in a river temple, all of the reign of Amenophis IV (or Akhenaten), the successor of Amenophis III, found at Tell el-Amarna in Egypt, and it seems quite possible in view of our discoveries that the plan of these el-Amarna buildings was taken from that of certain buildings, like the Beisan Amenophis III temple, in Syria and Palestine, for the close resemblance between them all is too great to be accidental. Further, we must remember the very intimate relationship that existed between the royal houses of Egypt and Western Asia about this time. In passing, it may be mentioned that the Beisan Amenophis temple also seems to bear certain analogies to the peculiar un-Babylonian form of cult room discovered in the archaic Ishtar temple at Assur, which dates to 2700 B.C. or a little earlier. Apart from the Egyptian influence which was still strong on the site, the other foreign influences of the period were (1) from the Ægean regions and (2) Syro-Hittite. The former is represented chiefly by much Cretan and Cypriote pottery and the latter by a magnificent bronze Hittite axehead, over forty Syro-Hittite cylinder seals, a cylinder seal with Hittite hieroglyphs and designs, and the Hittite-like appearance of the entrance to a great *migdol* or fort-tower situated near the temple. As to the Hittite influence it must be remembered that at about the time the Beisan Amenophis temple and *migdol* were built and according to the famous el-Amarna correspondence between Amenophis III and Amenophis IV of Egypt and their official governors and other peoples in Western Asia, the Hittites were intriguing against Egypt in North Syria and also that Hittite and Mitannian chiefs were already in Palestine. From the same correspondence we learn that bands of nomadic Semites, including the Khabiru and Suti, were already in Palestine. The former are identified by some with the Hebrews. As regards the direct Mesopotamian influence at Beisan during the period in

question, this seems still to be rather small although we must not forget that Babylonian writing was now in general use in Palestine. The only cuneiform inscription so far found in Beisan is that on a seal from the Amenophis III level; this seal is of lapis lazuli and dates about the XIXth or XVIIIth century B.C. It bears the words, "Manum, the diviner, and servant of the god Enki, *i.e.*, Ea."

Before the death of Amenophis IV (Akhenaten), in 1358 B.C., Palestine had passed entirely out of Egyptian control, the main cause being the king's fervid devotion to his religion, which made him neglect the affairs of his empire. After his death, kings Smenkhkara, Tutankhamen, Ai, Horemheb, and Rameses I succeeded one another on the throne of Egypt, which henceforth lost its control over the rich countries of Palestine and Syria until the time of Seti I (1313–1292 B.C.), the founder of the sixth city-level on the Beisan tell. Nothing is known of the history of Beisan during this period when no doubt local chieftains ruled over the city.

Palestine was reconquered by Seti I in the first year of his reign. During the course of his campaign he came to Beisan, where he erected a fort and also a temple. In the fort he erected two stelæ, one of which records the delivery of the city from invaders from the country on the east side of the Jordan. The text mentions Hamath, a city in the Yarmuk Valley, to the southeast of the Sea of Galilee; Pella, an ancient city opposite Beisan on the eastern side of the Jordan; Rehob, to the south of Beisan; and Yenoam, a place which may have been near Lake Huleh, north of the Sea of Galilee. It appears that Seti heard that the chief of Hamath had collected together many people, allied himself with the people of Pella, and attacked Beisan; he had also laid siege to Rehob. Thereupon the king sent the Army of Amen to Hamath, the Army of Ra to Beisan, and the Army of Sutekh to Yenoam, and overthrew the foes in "the space of a day." Whether the foes were Hittites there is at present no means of knowing. The other Seti stela seems to refer to another invasion by tribes from across the Jordan but its text is badly weathered.

It certainly does mention the Apiru, a people who are met with five times elsewhere. Some identify the Apiru with the Hebrews.

During the time of the next king, Rameses II, who also left a stela at Beisan, the main influence apart from the Egyptian seems to have been from the Mediterranean regions. The stela in question is not an important one, although it does contain a reference to the city of "Raamses" of *Exodus*, i, 11.

From the death of Rameses II in 1225 B.C. until the death of Rameses III in 1167 B.C. the Egyptians were probably in more or less constant control of Beisan and therefore there are many remains of their culture found there. Among these remains is a seated statue of Rameses III himself—a very important monument historically. Now during the whole of the periods of Egyptian rule at Beisan, that is, from the time of Thothmes III to Amenophis IV and from Seti I to Rameses III, the major part of the troops left in the fort must have been Mediterraneans, including the redoubtable Sherdenen and others who were officered by the Egyptians. Surely much of the Mediterranean influence in Beisan is due to the presence of these troops? The pottery anthropoid slipper-shaped sarcophagi in which some of these troops were buried have actually been found in the local cemetery; other similar examples have been found in the Nile Delta. All the sarcophagi have Mediterranean (including Ægean and Cypriote) objects with them.

When Rameses III died in 1167 B.C. the Mediterranean troops perhaps took possession of the Beisan citadel and later, about 1080 B.C., amalgamated with the incoming Philistines and others against whom they had formerly fought. Thus ended the Bronze Age, for the Philistines brought with them the extensive knowledge of the use of iron. The Philistines were no doubt, in part, oi the same race as some of the mercenaries and it therefore seems to me that the Philistine settlement in Palestine after the death of Rameses III can hardly be called as a whole a "conquest" in the strict sense of the word but more of an amalgamation between themselves

and their kinsmen who were already in the country in the shape of the mercenary troops. About 1020 B.C. the Philistines in Beisan defeated the Israelites upon Mount Gilboa to the southwest of that place and exhibited the body of Saul in the fortress. Later on, about 1000 B.C., the Israelites under King David seem to have taken the fort from the Philistines. With the Israelite conquest the last real influx of Mediterranean culture may be said to have stopped at Beisan, that is to say, so far as the Early Iron Age is concerned. As will be noticed, I have said nothing of the Mesopotamian or North Syrian influences at Beisan from the time of Seti I to the Philistine era, but as a matter of fact with the exception of the northern invasion from Hamath in the time of Seti I, very little is known about such influences.

In conclusion I should like to think that I have given you a fair account of what is so far known of the foreign influences at Beisan from the earliest times until the Philistine era, and for further details I would refer you to my forthcoming volume, *The Topography and History of Beth-shan*, to be published by the Museum of the University of Pennsylvania, by whom the site is being excavated. The chief surprise brought about by our excavations was to observe the predominance of the Egyptian and Mediterranean influences to the exclusion of the Mesopotamian influence.

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